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ART. I.—*On a New Form of Mountain or other Barometer;* by J. H. ALEXANDER, Esq.—(with a plate.)

THE modifications of shape and details, which different ingenious individuals have proposed or executed upon barometers, are now so numerous, that, if on the one hand one might be supposed dispensed from adding to the variety, equally on the other the want of full acceptation, with any, serves to shew the object yet unattained, and the whole subject therefore open still for reflection and effort. I question much if, generally, in the judgments of those who have had more especial occasion for making observations with mountain-barometers, all the modern complications of structure, introduced with the best intentions, are not found to have contributed to the embarrassment of the observations if not of the result; by rendering necessary a number of merely collateral operations and by hiding accidental defects, which are only least harmful when soonest found out.

Mr. Hassler seems to partake of an opinion like this, from the account which has been recently published* of the new portable barometer of his construction—an instrument characterized by all the originality and much of the appropriateness, which belong to all the works of this distinguished philosopher. I mention this instrument in particular, that I may not obtain credit for more novelty than really belongs to the arrangement which I propose. Touching the respective merits of either or of any other arrangement, it is not my purpose now to speak.

* Doc. 176, H. R. 2d session, 27th Congress.

In planning my barometer, besides the abstract principles which must of course govern in any ascertainment of the weight of the atmosphere directly by experiment, I had these two concrete ones for guides:—

1st. That the mercurial column should be throughout so exposed to view, that no fracture, irregularity, in-crept air or other accidental affection of its trustworthiness, can escape instant notice.

2nd. That the observer be left at all times free, to employ only so much of the auxiliary apparatus as fits the object he may have in view, or the refinement (not the certainty) he may aspire to. Hence nearly every one of the few pieces that compose the reading of the result is entirely detached, and to be added or removed at pleasure: in the case of its employment as a stationary barometer, this may be said of every piece.

The extent, to which these principles and others accessory to them have been carried out, is seen in fig. 1 of Plate IV, accompanying this description. The entire instrument is here shown, with all its parts in suitable positions for use. These parts are,

1. A small rectangular steel bracket, with a gimlet-pointed screw for clamping it to any convenient upright wooden support by one arm; while the other is pierced to allow the passage of the stem of the suspension hook.

2. The suspension hook, made of a single piece of steel; suitably bent at one end, the bearing part of which is also fashioned into a knife-edge; the other end being screw-cut and receiving, after its being passed through the bracket, a nut by which its vertical motion is produced and its position regulated. A washer intervenes between this nut and the bracket: but otherwise there is no provision for any azimuthal motion, such a motion, more than the nut itself has, being unnecessary. In elevating or depressing the tube, it is only required to prevent the hooked end from turning horizontally; by the finger, the revolutions of the nut then have all their proper effect.

3. The corve, or basket-handle, for carrying the tube. The rim and arms of this are cut out of a piece of clock-spring; the cross-bar of steel is a knife-edge corresponding with the bearing part of the hook; the excess in length of the rim over the circumference of the tube is bent from both ends equally to allow of forming clamp-jaws; one end is reinforced in thickness with a piece of steel riveted to it, in which a screw thread is cut; and

finally a clamp-screw, whose head is seen in the sketch, working through the jaws aforesaid, completes the arrangement, and allows of its being put on or taken off at pleasure. It is not advisable to interpose between the spring and the glass any paper, woollen cloth, or other hygrometric or electric substance.

4. The tube itself; as to its contents and their manner of introduction, I will speak hereafter. The tube should be about 35 inches long; in the sketch, in order to bring it within the limits of the page, I have represented it as much shorter. The open end is ground off as smoothly and as nearly at right angles to the tube-axis as may be; about an inch and a half from the end, a short line is marked to serve as the zero of the graduation; and at a half-inch or some other convenient length above, another mark is made for the subsequent adjustment of the float when the instrument is in use.

The graduation is then continued, either in the engine or by hand, so as to lay off in tenths of inches from 27 to 31 inches; and higher or lower in the discretion of the maker, *on the tube.* It is obvious that this division may, if so desired, be carried to hundredths in the same operation: but I have not found it desirable. Many tubes are sufficiently soft to be cut at once by a hard steel point in the engine: if the cutter be made of white cast iron of a kind frequently met with, hardly any tube will present a difficulty; but in any event, and in the particular case from which this description is taken, the resort to fluo-silicic acid to etch the lines is ready and successful. In whatever manner produced, the reflections from the mercury column behind them lend an increased depth and beauty.

5. The vernier is composed of a piece of watch-spring long enough to reach round three fourths of the circumference of the tube, and thus to hold in its place the scale, riveted at right angles to the spring. The lower edge of the spring is worked *true;* and forms an index-edge, similar in fact to what is generally used in mountain-barometers. The scale itself is made of mother of pearl; not so much from the luxury of taste, as for two practical reasons: the first, that the peculiar surface of pearl offers much less friction than any other material otherwise applicable, and thus makes the sliding of the vernier very uniform, easy and pleasant; and the second, that the transparency of the pearl allows the graduation of the tube to be seen through the scale of the vernier, and the

observation of coincidences therefore to be much more precise and agreeable.

The reading is not nearer than to 0.01 of inches; which is quite refined enough for any purpose to which I may expect its application, and indeed I might say for every purpose, except the determination of some atmospheric constants, when all recognized corrections would have to find their place. Whenever in such case or any other, all those corrections are employed, it will be appropriate to read the measurement closer and closer. At present there seems to be no need.

For instance, in the climate of Baltimore, for at least half the year, a change of 1° Fahr. in the dew-point implies an elevation or depression of the barometer of more than 0.016: such a change is very common, in the same locality, within the hour—such a difference highly probable, at the same moment, between places not very remote even from one another. Yet in the multitude of barometers read to thousandths, who reads the hygrometer?

So, in the application of the barometer to other than strictly meteorological purposes, the correction of a zenith distance for refraction when the angle is as great as 85°, is upon 0.01 of an inch of mercury only 0.2 of a second—a quantity to be sure generally admitted in calculation, but not materially affecting the most of astronomical or geodetic results. In zenith distances not so great, more usual, and more reliable, the amount of correction is still less: an altitude of 45° varies for $\frac{1}{100}$ in the barometer less than two hundredths of a second, a quantity in all ordinary calculations to be safely neglected. Similar considerations apply to this instrument, when rendered portable for the determination of heights. If the thermometer and hygrometer remain the same, a variation of $\frac{1}{1000}$ in the barometric column would correspond to a difference of level of little more than 10 inches; a space far within what any barometric observation has yet pretended to answer for.

The correction, too, arising from the specific gravity of the mercury, a particular rarely registered with the measurements, encloses within no narrow margin the apparent accuracy of a reading to thousandths of inches. If we take 13.6 as a mean specific gravity, giving with a certain temperature and dryness a stand of 30 inches, a specific gravity of 13.601 would give an equivalent stand under the same circumstances of only 29.9978 inches.

I apprehend it would be but accidental, if two barometers, made even by the same person but at different periods, or from any cause containing mercury of different lots, agreed in the specific gravity as nearly as this.

Finally, not to make too long discussion of such a point, it is even rare to find two observers, in direct succession upon the same instrument, reading to a coincidence as close as the thousandth of an inch. I thought myself therefore justified in discarding a graduation which, as I before said, multiplies the refinement without increasing the certainty.

Nevertheless, in limiting the tube-graduation to tenths of inches, I have not shut out the means of subdivision to thousandths in a ready and unexceptionable manner; whenever such subdivision should be requisite. These means consist in the application of a reading microscope or micrometer, whose position on the tube is held and regulated by a spring and clamp. The zero is adjusted, first, to the now magnified image of the mercury surface: and then, by the motion of the screw, the space between said surface and the nearest division on the tube below, is measured in hundredths of inches by the comb of the micrometer, and thousandths on the head of the screw in a well-known manner.

6. A piece of watch-spring, about three inches in length, is bent round at one end, so as to embrace three quarters of the circumference of the tube: the other end, which will then project from the tube, has merely a light triangular notch made in its upper edge to catch the loop of string or wire, which suspends the thermometer. In order to prevent any unsteadiness or wriggling motion, which would be otherwise likely in so narrow a strip, a piece of an inch length, from the same spring, is fastened with a single rivet at right angles to the former, so as to be vertical or nearly so when the clasp is made; or, as was done in the present instance, a cruciform piece is cut out of a wide clock-spring.

7. Nothing more need be said of the float than that it is of ivory, worked as thin and light as is consistent with its safety; and that the distance, between its under surface and the parallel fiducial edge of the rectangular notch seen in it, must be exactly equal with the space between the zero mark on the tube and that other line next above the zero mark, which has been spoken of already in § 4. It is obvious that in such case, when the image of said fiducial edge seen by reflection on the tube, coincides with the

appropriate mark, the under surface of the float and of course the surface of the mercury beneath it in the cistern must also coincide with the zero of the graduation.

I apprehend that this symmetrical position of the float around the tube, tends to make such a coincidence of the zero mark and the mercury-surface more exact than is likely to be attained in any other arrangement; which places the float at a greater distance, and entirely on one side of the tube. No single point of the mercury-surface, perhaps, is ever to be taken as precisely in the normal plane of the base to the atmospheric column equilibrating the barometric column: but this equilibrium is made by pressure of an infinite number of atmospheric columns whose bases are in different horizontal planes, or, what is the same, by the pressure of an aggregate column whose base is as irregular as the cistern surface. A float, then, whose horizontal base extends equally all round the axis of equilibrium, may be supposed to present the fairest average of these irregularities and a general resultant of these several pressures.

In any event, its exactitude may be taken as within any such methods as the estimation of the capacity of the cistern, an English manner of construction; or the ivory point used in some continental barometers; or the *minimum visibile*, the most used but I think the most objectionable of all.

8. The cistern is a porcelain or glass dish of suitable size; which, when occasionally cleaned, admits of the application of heat to drive off all moisture. Wood is not favorable for such a purpose; because it receives a very smooth surface only with difficulty. It is besides hygrometric: and all varnish which might be used to remedy this disqualification brings another quite as bad —in the action, which the resinous components of such varnish are apt to have on the mercury.

What has been so far described, contains all the parts necessary for establishing a stationary barometer. I suppose, of course, though I have not yet mentioned it, that the mercury is pure, that its specific gravity is ascertained, that the tube has been boiled, and that in immersing it in the cistern no substance has been placed in contact with the mercury, likely to act upon or soil it. As a suitable implement for this purpose, I have figured in fig. 5 a tool, which Mr. Green applies in such cases. It is of iron, covered with clean undyed leather: the spheroidal pad at

one end takes the place of the finger; and the pressure, which from its shape can be exerted by the whole hand and wrist to keep it in place, will be, in several positions of the tube, of the highest convenience.

The additions, necessary for constituting a mountain barometer, are shewn in figs. 2 and 3; of which the first is a vertical section, and the other a ground plan seen from below, of the appliances.

In fig. 2, is seen surrounding the tube, shewn by dotted lines, a steel cylinder, cemented to the tube. The middle part of said cylinder is left more massive than either extremity, and is worked into an octagonal shape to allow its being firmly clamped in a vice. The lower extremity is cut into a screw thread, which fits the screw of the inverted, bottomless, iron disk that is to form the termination of the tube. This disk is made of a piece of a gun barrel. The lower edge of this is also screw-cut, and is fitted with the ring seen in fig. 3. The notches in that ring, which are also shewn, serve for catching a lever or handle by which the ring is screwed up. The single line in fig. 2, between the ring and the disk, is a section of the bottom of the sub-cistern; which bottom is a plate of Russia-iron, hammered so thin as to be flexible, and secured by the ring before mentioned.

One side of the disk is tapped with a screw-thread and furnished with a screw, as seen in both drawings, for opening or shutting off the communication of the atmosphere. As this is only opened, when the tube is immersed in the cistern; and as one's fingers should be carefully kept from the mercury, fig. 4 represents a tool which I use for unscrewing and screwing: the hooked end of it, as can be readily imagined, fitting in the hole of the handle seen in fig. 3. Inasmuch as a great deal of power cannot be exerted with such a tool, the handle is necessary in order to tighten up the screw more effectually after the withdrawal of the barometer from the cistern.

I should mention that all the permanent screws—those connecting the disk with the cylinder or collar, and the ring with the disk—are laid in a cement, which Mr. Green contrived, and which, fusible only at a very high heat, has the property of quickly hardening. An idea of the tightness thus given to all the joints may be formed, when I state that in an experimental arrangement of this kind, wherein the disk had been turned out of

a piece of cast-iron, supposed to be uniform enough,—the tube being inverted and immersed in water of about 130°,—the expanded mercury, not able to escape by any of the joints, actually forced its way in two or three places through the metal itself; that is, through flaws in its texture, though they were so microscopic as not to be observable to the naked eye, except by the emission of the quicksilver. So comminuted was the stream, and so great the force of emission, that the metal ascended in graceful wreaths, like smoke, to a height not less than two feet.

Having now explained the different parts of the instrument, I shall describe the various steps which were taken in the composition of it.

First, upon the open end of the tube was cemented the collar; and they were then ground to remove any irregularities, and make them fit evenly. Then the tube was clamped in a vice; and the disk was screwed on in cement, as far as possible. Clean mercury was then sifted into the tube, until it filled a part of the disk. The whole was then boiled over a spirit-lamp by successive portions; the flame being finally brought about as near to the end of the tube, as is shewn in fig. 2. The whole was then replaced in the vice, the plate of Russia-iron laid in its place, and the ring screwed on in cement, as tightly as might be. The tube was then removed, the side-screw taken out, while the instrument was held in a position somewhat inclined, but not so far as by possibility to uncover the end of the tube by the mercury now contained in the disk; and warm mercury was sifted down through the screw-hole, until it overflowed. The overflowing was regulated by the pressure of the finger against the elastic bottom, the tube being held nearly horizontal; and the screw was inserted and turned tight. I should have said that due care was taken, before inserting the screw, to disentangle and expel any air that might have been taken in with the mercury; and the proof of success in that regard was afforded, in giving the instrument, held in proper position, some smart shocks in order to see if any air could be forced up the tube above the cylinder. None shewed itself; and I think because there was none there.

This mode of terminating the tube, I regard as one of the most important modifications which it has been my aim to describe; and the merit of its suggestion belongs to Mr. Green, the artist who constructed the various portions of the instrument. The

end which we had in view, in various plans which were contrived and tried, was to provide some means of following the expansions and contractions of the mercury, arising from changes of temperature; so as to furnish the air no chance of getting in. This the elastic plate does very well; indeed I may say completely: the heat of the hand being enough, in a few minutes, to cause a convex appearance, and the pressure of the atmosphere, on its removal to a cold room, sufficient to make it concave.

It was of course interesting, to have the plate, and by consequence the disk, of as small a diameter as would be compatible with the other conditions requisite. After some experiments, therefore, to ascertain what elasticity was attainable by hammering out the Russia-iron, consistent with uniformity and strength, I calculated the following table, shewing the relations existing between the clear inside diameter of the disk and that of the tube, on the assumption that the range of temperature does not exceed 90° Fahr., and that the mercury is always pressed upon by the plate.

Inside tube-diameter. Inch.	Inside diameter of disk, or exposed plate diameter. Inches.
0.20	0.90
0.25	1.13
0.30	1.35
0.40	1.81
0.50	2.26
0.60	2.71
0.70	3.16
0.75	3.39

These proportions have been found to suit very well in practice in the instances where they have been applied.

I have before spoken of the advantage in precision, which the symmetrical disposition of the float is presumed to afford to the measurements. I give here the formula, for calculating the amount of correction which is to be applied to make up for the partial sinking of the float, and the consequent elevation of the mercury in the tube and cistern.

It is manifest, that the heavier the float, with the same diameter, the deeper it (and of course the zero of the graduation) will sink in the cistern; and also, that with a float of constant weight

and diameter, the larger the cistern the less will be the vertical rise of the fluid in it, in consequence of the displacement by the float. If then we call

W, the weight of the float—for instance in grains;

w, the weight of a cubic inch of mercury at any given temperature;

Δ, the diameter of the cistern in inches;

D, the diameter of the float "

d, the external diameter of tube "

δ, the internal diameter " "

And π, the circumference of a circle whose diameter is unity; the sum of the correction will be

$$-\left(\frac{W}{w}\cdot\left(\frac{1}{\frac{\pi}{4}\cdot(D^2-d^2)}+\frac{1}{\frac{\pi}{4}\cdot(\Delta^2+\delta^2-D^2)}\right)\right);$$

reducing to a common denominator, we have

$$-\frac{W\cdot(\Delta^2+\delta^2-d^2)}{\frac{\pi}{4}\cdot w\cdot(D^2-d^2)\cdot(\Delta^2+\delta^2-D^2)}.$$

If we neglect the change of specific gravity by a change of temperature, as we may in this case safely do, and take $w=3426.56$ grs. (which corresponds to a specific gravity for mercury of 13.5728 at 62° F., and very near 13.6 at the maximum density of water,) and give to π its numerical value of 3.14156, etc., the correction becomes in round numbers

$$-\frac{W\cdot(\Delta^2+\delta^2-d^2)}{2691\cdot(D^2-d^2)\cdot(\Delta^2+\delta^2-D^2)}.$$

Such is the quantity to be subtracted from the reading, in order to give the height of the barometric column unaffected by the weight of the float.

It only remains to be added, that in comparing the few barometers of this construction, which have so far been made, with some others, among which were those of Mr. Troughton, of Mr. Hassler, and of Mr. Green—I mean coming from the hands of the persons named—the former, after deducting the amount of correction, stood uniformly higher than any of the latter; which I attribute not so much to the individual precautions in securing a better vacuum as to the greater precision with which the zero of the measurement can be ascertained.

Baltimore, Md., May 25, 1843.

ART. II.—*Notice of "Molluskite," or the fossilized remains of the soft parts of Mollusca;* by GIDEON ALGERNON MANTELL, Esq., LL. D., F. R. S., G. S., &c.

[Read before the Geological Society of London, January, 1843.]

SINCE the interesting discovery by Dr. Buckland of the nature and origin of the fossil remains termed *coprolites*, substances having the same general appearance and composition, but destitute of the spiral structure, and distributed in amorphous masses in the strata, have commonly been placed in the same category under the name of *pseudo-coprolites.*

In the blocks of *firestone* or upper green sand, which are seen at low water along the shore at Southbourne in Sussex, concretions of this kind are thickly interspersed among the shells which abound in those rocks. In my earliest geological researches along the Sussex coast, these fossil bodies particularly arrested my attention, but I failed to obtain any clue to their origin, until the important memoir on coprolites by Dr. Buckland, pointed out the right path of enquiry, and offered a satisfactory solution of a problem which had baffled the attempts of previous observers.

That a large proportion of the concretionary and nodular masses of the substance in question is the mineralized egesta of fishes and other marine animals, there can be but little doubt; although it is rarely possible to detect the traces of intestinal structure which are so commonly impressed, more or less distinctly, on the coprolites of the chalk, wealden, and lias. But in the rocks at Southbourne, instances are not unfrequent in which the coprolitic matter (I use the term for the sake of convenience) occurs in the state of casts of shells belonging to the genera Cucullæa, Venus, Trochus, &c., which abound in the firestone; and in these examples the substance appears to have originated from the soft bodies of the mollusca. In Sussex, in the layers of firestone which occur at the line of junction with the galt, pseudo-coprolites are very abundant. They are not uncommon in the beds of galt, at Ringmer and Norsington near Lewes, and at Bletchingly in Surrey; and they abound in the same argillaceous deposit at Folkstone in Kent. Dr. Fitton, in his elaborate memoir on the strata below the chalk, (Geological Transactions, Vol. IV, part 2, page 111,) has given an accurate description and analysis of

the coprolitic nodules and concretions which occur at Folkstone. Dr. Fitton states, "that they resemble coprolites in their chemical composition, though no traces of animal structure are apparent in them. They sometimes enclose portions of shells, but no fragments of bone or scales of fishes have been detected. In some cases they are of a very irregular figure, surrounding or incorporated with fossil remains, especially of ammonites, *the anterior of which is filled up with matter of the same kind.*" The last quoted remark of this eminent geologist, bears immediately on the subject of the present communication.

In the grey Shanklin sand these substances also abound in some localities. I have observed them in western Sussex, in Surrey, in the Isle of Wight near Ventnor, and in Kent. But in no locality do they occur in greater number and variety, than in the "Iguanodon quarry" of Kentish rag near Maidstone, belonging to Mr. W. H. Beusted, to whose talents, zeal, and liberality, geology is indebted for many important discoveries.

Mr. Beusted having long paid attention to this subject, had the kindness to submit to my examination (more than two years since) several specimens of Rostellariæ, Trigoniæ, Cucullæa, and other shells, the cavities of which were filled with a dark brown substance, in every respect identical with the nodular and irregular concretions of coprolitic matter, which abound in the surrounding sandstone. At the same time Mr. Beusted expressed his conviction that the carbonaceous substance was derived from the soft bodies of the mollusca, and that the concretionary and amorphous portions of the same matter, dispersed throughout the sandstone of this bed, were fossilized masses of the soft bodies of the animals which had become disengaged from their shells, and floated in the sea, till enveloped in the sand and mud, which is now concreted into the sandstone called Kentish rag. The evidence collected by Mr. Beusted appears to me so conclusive, and so confirmatory of the correctness of the opinion I have previously advanced, that I beg to place before the Society the following abstract of his correspondence with me on the subject.

"The bed of Kentish rag in my quarry which lies immediately beneath the stratum that contained the remains of the Iguanodon, abounds in the usual shells of the Shanklin sand, particularly in Trigoniæ, (generally T. alæformis,) and there is an abundance of a dark brown coprolitic looking matter, of which I send

you specimens. In some instances this material actually forms the entire casts of the univalves and bivalves, and I think there can be no doubt that it is derived from the soft bodies of the animals which inhabited the shells found in connection with it, fossilized in this peculiar manner. There are many examples which look more like true coprolites of fishes, and some of these contain shells partly crushed, as if they had been the partially digested contents of the intestinal canal. I am therefore inclined to think that the dark material which now occupies the shells, was the soft body of the mollusc, that those of a concretionary form which are imbedded in the stone are coprolites, and that the shapeless portions of this substance distributed in the rock have originated from floating masses of dead shell-fish. In illustration of the manner in which such an accumulation of materials as I find in my quarry, may have been formed, I beg to call your attention to the following extract from the American Journal of Science for 1837, which seems to me to afford an explanation of some of the appearances that I have attempted to describe.

"'One of the most curious phenomena of the year 1836, was the fatal effect of an epidemic among the molluscous animals or shell fish of the Muskingum River, in the state of Ohio. It commenced in April and continued until June, destroying millions of that great race, which peoples the beds of streams. As the animals died, the valves of the shells opened, and decomposition commencing, the muscular adhesions gave way and the fleshy portions rose to the surface of the water, leaving the shells in the bed of the stream. As masses of the dead bodies floated down the current, the headlands of islands, piles of fixed drifted wood, and the shores of the river in many places were covered with them, and the air in the vicinity was tainted with the putrid effluvia exhaling from these accumulations of decomposing animal matter. The cause of the disease among the shelly tribes remains as much a mystery as that of the Asiatic cholera among the human race.'

"Now nearly the whole of the shells which occur in the bed of Kentish rag, appear to have been dead shells. I mean that from the open state of the valves it is probable that the animals for the most part, were dead before they were enveloped in the sand and mud; and from the large quantity of water-worn fragments of wood perforated by lithodomi, that is imbedded with them, it

would seem that this stratum had constituted a bank of drifted wood and shells, presenting a very analogous condition to the phenomena above described. The gelatinous bodies of the Trigoniæ, Gervilliæ, Ostreæ, Rostellariæ, &c. detached from their shells may have been intermingled with the drift wood, in a sand bank, while in some instances the animal matter would remain in the shells, and become fossilized in the state observable in the accompanying examples."

The above remarks present a correct view of the circumstances under which the phenomena referred to occur in the quarry of Mr. Beusted.

Some of the dark substance extracted from a Trigonia, was submitted by my friend, the Rev. J. B. Reade, to a careful analysis by Mr. Rigg, who obligingly favored me with the following note: "My analysis confirms your suspicion respecting the presence of *animal carbon* in the substance which you sent for examination. After removing the lime, &c. by means of dilute hydrochloric acid from ten grains of the darker portion of the stone, there remained 1.2 grains of dark powder, which gave by analysis with oxide of copper .16 of a cubic inch of carbonic acid, and *apparently* a small portion of nitrogen. On subjecting to the same kind of analysis two grains of the darker body without previously acting upon it by any acid, .054 of a cubic inch of carbonic acid was obtained; so that from these results there is no doubt but the darker portion of these substances contains about .35 per cent. of its weight of carbon in an organized state."

The presence of animal carbon in fossil remains will, I expect, be found of frequent occurrence, not only enclosed in the shells of mollusca, but disseminated in the surrounding matrix. In the unique specimen of a fossil fox from Ruingen, which I had the pleasure of dissecting for our distinguished president, I found a considerable quantity of carbonaceous coprolitic matter within the abdominal region, and I have frequently detected its presence in the grit and sandstone of Tilgate Forest associated with the bones of reptiles. The black material which is so commonly seen to occupy some of the spiral cavities of the Paludina composing the Sussex and Purbeck marbles, and which by its contrast with the white calcareous spar, adds considerably to the beauty of those fresh-water limestones, contains a large proportion of animal carbon, doubtless derived from the soft parts of

the mollusca having been enclosed at the period of its formation. A microscopical examination detects with a low power innumerable portions of the nacreous laminæ of the shells of extreme thinness, intermingled with the carbonaceous matter, together with numerous siliceous spiculæ of sponges, very minute spines of echinoderms and fragments of Polyparia; these extraneous bodies probably became entangled among the soft animal matter before the latter had undergone decomposition.

If my inferences be deemed correct, the term *molluskite* would be a proper designation for the substance in question.

N. B. This memoir was illustrated by drawings and numerous specimens of the molluskite, in some instances forming large amorphous carbonaceous masses in the sandstone, and in others filling the shells of the Trigoniæ, Terebratulæ, &c.*

Crescent Lodge, Clapham Common, (Eng.) January, 1843.

ART. III.—*An effort to refute the arguments advanced in favor of the Existence, in the Amphide Salts, of Radicals consisting, like Cyanogen, of more than one element;* by ROBERT HARE, M. D., Prof. Chem. Univ. Pennsylvania.

(Concluded from p. 65.)

46. Respecting the new principles which I have been contesting, Dr. Kane alleges "that the elegance and simplicity with which the laws of saline combination may be traced from them is remarkable," because he conceives, that without an appeal to those principles, the fact that the number of equivalents of acid in a salt are proportionable to the number of equivalents of oxygen in the base, would be inexplicable.

47. Thus, when the base is a protoxide, we have one atom of the protoxide of hydrogen to take its place; when the base is a sesquioxide (two of radical and three of oxygen,) three atoms of the protoxide of hydrogen take its place: if the base be a bioxide, two atoms of the protoxide of hydrogen take its place.

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* Dr. Mantell has been so kind as to forward to us, in illustration of his memoir, very distinct and satisfactory specimens of the *molluskite*, together with ammonites from the Kimmeridge clay, having elongated beaks in good preservation; also belemnites with their chambers preserved.—EDS.

48. I have already adverted to the existence of certain chemical laws, inexplicable in the present state of human knowledge. Among these is that of the necessity of oxidation to enable metallic radicals to combine with acids. But as a similar mystery exists as respects the adventitious property of combining with radicals, which results from the acquisition of an additional atom of oxygen by any of the compounds hitherto considered as anhydrous acids, the new doctrine has in that respect no pre-eminent claim to credence.

49. But if, without impairing the comparative pretensions of the prevailing doctrine, we may appeal to the fact that the acquisition of an atom of oxygen confers upon a radical the basic power to hold one atom of acid, is it not consistent that the acquisition of two atoms of oxygen should confer the power to hold two atoms of acid, and that with each further acquisition of oxygen a further power to hold acids should be conferred?

50. So far then there is in the old doctrine no more inscrutability than in that which has been proposed as its successor. Since if on the one hand it be requisite that for each atom of oxygen in the base, there shall be an atom of acid in any salt which it may form, on the other, in the case of the three oxyphosphions, for each additional atom of hydrogen extraneous to the salt radical, there must be an atom of oxygen superadded to this radical.

51. It being then admitted that, numerically, the atoms of acid in any oxysalt will be as the atoms of oxygen in the base, it must be evident that whenever an oxysalt of a protoxide is decomposed by a bioxide, there will have to be two atoms of the former for one of the latter. For the bioxide has two atoms of oxygen, and requires by the premises two atoms of acid, while the salt of the protoxide, having but one atom of oxygen, can hold, and yield, only one atom of acid. Two atoms of this salt, therefore, whether its base be water, or any other protoxide, will be decomposed by one atom of bioxide; provided the affinity of the acid for the bioxide predominate over that entertained for the protoxide, as when water is the base.

52. It follows, that the displacement of water from its sulphate, adduced by Kane, does not favor the idea that hydrous sulphuric acid is an oxysulphionide of hydrogen, more than the impression that it is a sulphate of water.

53. Of course, in the case of presenting either a sesquioxide, or a trioxide, to the last mentioned sulphate, in other words, hydrous sulphuric acid, the same rationale will be applicable.

54. The next argument advanced by Dr. Kane, is, that *some* of the acids of which the existence is assumed upon the old doctrine, are hypothetical, as they have never been isolated. This mode of reasoning may be made to react against the new doctrine with pre-eminent force, since *all* of the compound radicals imagined by it are *hypothetical—none* of them having been isolated.

55. The third argument of the respectable author above named is, that acids display their acid character in a high degree only when in the combination with water.

56. This argument should be considered in reference to two different cases, in one of which all the water held by the acid is in the state of a base, while in the other an additional quantity is present acting as a solvent. So far as water, *acting as a solvent*, facilitates the reaction between acids and bases, it performs a part in common with alcohol, ether, volatile oils, resins, vitrifiable fluxes, and caloric. Its efficacy must be referred to the general law, that fluidity is necessary to chemical reaction. "Corpora non agunt nisi soluta."

57. In a majority of cases, basic water, unaided by an additional portion acting as a solvent, is quite incompetent to produce reaction between acids and other bodies. Neither between sulphuric acid and zinc, between nitric acid and silver, nor between glacial or crystallized acids and metallic oxides, does any reaction take place without the aid of water acting as a solvent, and performing a part analogous to that which heat performs in promoting the union of those oxybases with boric, or silicic acid.

58. It is only with *soluble* acids that water has any efficacy. The difference between the energy of sulphuric and silicic acid, under the different circumstances in which they can reciprocally displace each other, is founded on the nature of the solvents which they require, the one being only capable of liquefaction by water, the other by caloric.

59. In support of his opinions the author adverts to the fact, that with hydrated sulphuric acid, baryta will combine energetically *in the cold*, while a similar union between the anhydrous vapor and the same base cannot be accomplished *without heat*. But it ought to be recollected, that to make this argument good,

it should be shown wherefore heat causes the baryta, a perfectly fixed body, to unite more readily with an aëriform substance in which increase of temperature must, by rarefaction, diminish the number of its particles in contact with the solid. If the only answer be, that heat effects some mysterious changes in affinity, (or as I would say in the electrical state of the particles,) it should be shown that the presence of water or any other base has not been productive of a similar change, before another explanation is held to be necessary. But I would also call to mind that the hydrated acid is presented in the liquid state; and if it be asked why water, having less affinity than baryta, can better cause the condensation of the acid, I reply, that it is brought into contact with the acid both as a liquid and a vapor, of neither of which forms is the earthy base susceptible. But if all that is necessary to convert anhydrous sulphuric acid into an oxysulphionide, be an atom of oxygen and an atom of metal, what is to prevent baryta and anhydrous sulphuric acid from forming an oxysulphionide of barium? All the elements are present which are necessary to form either a sulphate or oxysulphionide; and I am unable to conceive wherefore the inability to combine does not operate as much against the existence of radicals as of bases.

60. I would be glad to learn why, agreeably to the salt radical theory, anhydrous sulphuric acid unites with water more greedily than with baryta, and yet abandons the water promptly on being presented to this base. Why should it form an oxysulphionide with hydrogen more readily than with barium, and yet display, subsequently, a vastly superior affinity for barium?

61. It seems to be overlooked, that anhydrous sulphuric acid, being the oxysulphion of the sulphites, ought to form *sulphites* on contact with metals.

62. But if the sulphate of water owe its energy to that portion of this liquid, which, by its decomposition gives rise to the compound radical oxysulphion, and not to the portion which operates as a solvent, wherefore in the concentrated state, will it not react with iron and zinc, without additional water, when, with dilution, it reacts most powerfully with those metals.

63. Some stress has been laid upon the fact, that sourness is not perceived, excepting with the aid of water, as if to derive force for the new doctrine from that old and popular, though now abandoned test of acidity; but it should be recollected that it is not the

water which goes to form the compound element in the "*hydracids*," erroneously so called, which confers sourness. Will any one pretend that either sulphuric or nitric acid, when concentrated, is sour? Are they not caustic? Can any of the crystallized organic acids be said to have a sour taste, independently of the moisture of the tongue? The hydrated oily acids being incapable of uniting with water as a solvent, have none of these vulgar attributes of acidity. The absence of these attributes in prussic acid would alone be sufficient to render it inconsistent to consider them as having any connexion with the presence of hydrogen.

64. It has been remarked, that liquid carbonic acid does not combine with oxides on contact. To this I would add, that it does not combine with water under those circumstances, but, on the contrary, separates from it like oil, after mechanical mixture: nor does it, under any circumstances, unite with an equivalent proportion of water to form a hydrate. Of course, as it is not to basic water that it is indebted for its ability to become an ingredient in salts, it cannot be held that this faculty is the result of its previous conversion into an *oxycarbionide of hydrogen*.

65. Chromic acid is admitted not to require water for isolation, and cannot, therefore, be considered as oxychromionide of hydrogen. Yet the oil of bitter almonds, which consists of a *compound radical*, benzule, and an atom of hydrogen, and which is therefore constituted precisely as the salt radical doctrine requires for endowment with the attributes of an "hydracid," is utterly destitute of that acid reaction which hydrogen is represented as peculiarly competent to impart. It follows that we have, on the one hand, in chromic acid, a compound endowed with the attributes of acidity, without being a hydruret of any compound radical; and, on the other, in oil of bitter almonds, a hydruret of a compound radical, without any of the attributes of acidity.

66. The last argument in favor of the existence of salt radicals, which I have to answer, is that founded on certain results of the electrolysis of saline solutions.*

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* It is well known that Faraday employed a very simple instrument to ascertain the quantity of the gaseous elements of water yielded in a given time, by a liquid subjected to the voltaic current. It consisted of a graduated tube, through the cavity of which the current was conveyed by wires, so terminating within it, as to have an interval between them through which the current, being convey-

67. On subjecting a solution of sulphate of soda to electrolysis, so as to be exposed to the current employed, simultaneously with some water in a voltameter, Daniell alleges that, for each equivalent of the gaseous elements of water evolved in the voltameter, there was evolved at the cathode and anode, not only a like quantity of those elements, but likewise an equal number of equivalents of soda and sulphuric acid. This he considers as involving the necessity, agreeably to the old doctrine, of the simultaneous decomposition of two electrolytic atoms in the solution, for one in the voltameter; while, if the solution be considered as holding oxysulphionide of sodium, instead of sulphate of soda, the result may be explained consistently with the law ascertained by Faraday. In that case, oxysulphion would be carried to the anode, where, combining with hydrogen, it would cause oxygen to be extricated, while sodium, carried to the cathode, and deoxidizing water, would cause the extrication of hydrogen.

68. Dr. Kane, alluding to the experiments above mentioned, and some others which I shall mention, alleges that "*Professor Daniell considers the binary theory of salts to be fully established by them.*

69. Notwithstanding the deference which I have for the distinguished inventor of the constant battery, and disinclination for the unpleasant task of striving to prove a friend to be in the wrong, being of opinion that these inferences are erroneous, I feel it to be my duty, as a teacher of the science, to show that they are founded upon a misinterpretation of the facts appealed to for their justification.

70. It appears to me, that the simultaneous appearance of the elements of water, and of acid and alkali, at the electrodes, as above stated, may be accounted for, simply by that electrolyzation of the soda, which must be the natural consequence of the exposure of the sulphate of that base in the circuit. I will in support of the exposition which I am about to make, quote the language

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ed by the electrolytic process, effected the decomposition of the intervening liquid, the resulting gas being caught and measured by the tube. This instrument has been called a volta-electrometer, or voltameter.

Faraday found that when various substances were electrolyzed, a voltameter being at the same time in the circuit, that for every equivalent of water decomposed within the tube, neither more nor less than an equivalent of the other body could be decomposed.

of Professor Daniell, in his late work, entitled, "Introduction to Chemical Philosophy," page 413:—

"Thus we may conceive that the force of affinity receives an impulse which enables the hydrogen of the first particle of water, which undergoes decomposition, to combine momentarily with the oxygen of the next particle in succession; the hydrogen of this again, with the oxygen of the next; and so on till the last particle of hydrogen communicates its impulse to the platinum, and escapes in its own elastic form."

71. The process here represented as taking place in the instance of the oxide of hydrogen, takes place, of course, in that of any other electrolyte.

72. It is well known, that when a fixed alkaline solution is subjected to the voltaic current, that the alkali, whether soda or potassa, is decomposed; so that if mercury be used for the cathode, the nascent metal, being protected by uniting therewith, an amalgam is formed. If the cathode be of platinum, the metal, being unprotected, is, by decomposing water, reconverted into an oxide as soon as evolved. This shows, that when a salt of potassa or soda is subjected to the voltaic current, it is the alkali which is the primary object of attack, the decomposition of the water being a secondary result.

73. If in a row of the atoms of soda, extending from one electrode to the other, while forming the base of a sulphate, a series of electrolytic decompositions be induced from the cathode on the right, to the anode on the left, by which each atom of sodium in the row will be transferred from the atom of acid with which it was previously combined, to that next upon the right, causing an atom of the metal to be liberated at the cathode; this atom, deoxidizing water, will account for the soda and hydrogen at the cathode. Meanwhile the atom of sulphate on the left, which has been deprived of its sodium, must simultaneously have yielded to the anode the oxygen by which this metal was oxidized. Of course the acid is left in the hydrous state, usually called free, though more correctly esteemed to be that of a sulphate of water.

74. I cannot conceive how any other result could be expected from the electrolysis of the base of sulphate of soda, than that

which is here described. Should any additional illustration be requisite, it will be found in a note subjoined.*

75. I will, in the next place, consider the phenomena observed by Professor Daniell, when solutions of potassa and sulphate of copper, separated by a membrane, were made the medium of a voltaic current.

76. Of these I here quote his own account—(Philosophical Magazine and Journal, Vol. xvii, p. 172)—

"A small glass bell, with an aperture at top, had its mouth closed by tying a piece of thin membrane over it. It was half filled with a dilute solution of caustic potassa, and suspended in a glass vessel containing a strong neutral solution of sulphate of copper, below the surface of which

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* It is easy to understand how a simultaneous appearance of oxygen and acid at the anode, and soda and hydrogen at the cathode, may ensue, simply by the electrolyzation of the alkaline base from the following association of formulæ.

Anhydrous sulphuric acid is represented by the usual formula, SO_3; oxygen by the usual symbol, O; sodium by Na; water, acting as a solvent, by HO. Each atom of oxygen, sodium, or acid, is numbered from right to left, 1, 2, 3, 4, so that the change of position consequent to electrolysis may be seen.

	1.	2.	3.	4.	Water.		
Anode	O	O	O	O	HO	Cathode.	
	1.	2.	3.	4.			
	Na	Na	Na	Na			
	1.	2.	3.	4.			
	SO_3	SO_3	SO_3	SO_3			
	HO	HO	HO	HO			
	1.	2.	3.	4.			
Anode	O	O	O	O	O	H	*Cathode.*
		1.	2.	3.	4.		
		Na	Na	Na	Na		
		2.	3.	4.			
	SO_3	SO_3	SO_3	SO_3			
	HO	HO	HO	HO			

As the atoms are situated in the second arrangement, the atom of oxygen (1), is at the anode, the atom of sodium, $\overset{1.}{\widetilde{Na}}$, with which it had been united, having been transferred to the second atom of sulphuric acid, which had yielded its sodium to the third atom of acid, $\overset{3.}{\widetilde{SO_3}}$, this having, in like manner, yielded its sodium to the fourth atom of acid, $\overset{4.}{\widetilde{SO_3}}$, from which the fourth atom of sodium, $\overset{4.}{\widetilde{Na}}$, had been abstracted by the electrolytic power. The atom of sodium thus removed from the fourth atom of acid, is represented in union with the oxygen of an atom of water, of which the hydrogen, H, is at the cathode.

it just dipped. A platinum electrode, connected with the last zinc rod of a large constant battery of twenty cells, was placed in the solution of potassa; and another, connected with the copper of the first cell, was placed in the sulphate of copper immediately under the diaphragm which separated the two solutions. The circuit conducted very readily, and the action was very energetic. Hydrogen was given off at the platinode in a solution of potassa, and oxygen at the zincode in the sulphate of copper. A small quantity of gas was also seen to rise from the surface of the diaphragm. In about ten minutes the lower surface of the membrane was found beautifully coated with metallic copper, interspersed with oxide of copper of a black color, and hydrated oxide of copper of a light blue.

"The explanation of these phenomena is obvious. In the experimental cell we have two electrolytes separated by a membrane, through both of which the current must pass to complete its circuit. The sulphate of copper is resolved into its compound anion, sulphuric acid + oxygen (oxysulphion), and its simple cathion, copper: the oxygen of the former escapes at the zincode, but the copper on its passage to the platinode is stopped at the surface of the second electrolyte, which for the present we may regard as water improved in its conducting power by potassa. The metal here finds nothing by combining with which it can complete its course, but being forced to stop, yields up its charge to the hydrogen of the second electrolyte, which passes on to the platinode, and is evolved.

"The corresponding oxygen stops also at the diaphragm, giving up its charge to the anion of the sulphate of copper. The copper and oxygen thus meeting at the intermediate point, partly enter into combination, and form the black oxide; but from the rapidity of the action, there is not time for the whole to combine, and a portion of the copper remains in the metallic state, and a portion of the gaseous oxygen escapes. The precipitation of blue hydrated oxide doubtless arose from the mixing of a small portion of the two solutions."

77. It will be admitted, that agreeably to the admirable researches of Faraday, there are two modes in which a voltaic current may be transmitted, *conduction* and *electrolyzation*. In order that it may pass by the last mentioned process, there must be a row of anions and cathions forming a series of electrolytic atoms extending from the cathode to the anode. It is not necessary that these atoms should belong to the same fluid. A succession of atoms, whether homogeneous, or of two kinds, will answer, provided either be susceptible of electrolyzation. Both of the liquids resorted to by Daniell, contained atoms susceptible of

being electrolyzed. If his idea of the composition of sulphate of copper, and the part performed by the potassa, were admitted for the purpose of illustration, we should, on one side of the membrane, have a row of atoms consisting of oxysulphion and copper; on the other, of oxygen and hydrogen.

78. Recurring to Daniell's own description of the electrolyzing process, above quoted, an atom of copper near the anode being liberated from its anion, oxysulphion, and charged with electricity, seizes the next atom of oxysulphion, displacing and charging an atom of copper therewith united. The cupreous atom thus charged and displaced, seizes a third atom of oxysulphion, subjecting the copper, united with it, to the same treatment as it had itself previously met with. This process being repeated by a succession of similar decompositions and recompositions, an electrified atom of copper is evolved at the membrane, where there is no atom of oxysulphion. Were there *no other anion* to receive the copper, evidently the electrolyzation would not have taken place; but oxygen, on the one side of the membrane, must succeed to the office performed by oxysulphion on the other side; while hydrogen, in like manner, must succeed to the office of the copper.

79. Such being the inevitable conditions of the process, how can it be correctly alleged by Professor Daniell, the transfer of the copper being arrested at the membrane, that as this metal "*can find nothing to combine with*," it gives up its electrical charge to the hydrogen, which proceeds to the cathode? As hydrogen cannot be present, excepting as an ingredient in water, how can it be said that the copper can discharge itself upon the hydrogen, without combining with the oxygen necessarily liberated at the same time by the electrolytic process? How could the copper, in discharging itself to a cathion, escape a simultaneous seizure by an anion? Would not the oxidizement of this metal be a step indispensable to the propagation of that electrolytic process, by which alone the hydrogen could, as alleged, "*pass to the platinode*," i. e. cathode?

80. In these strictures I am fully justified by the following allegations of Faraday, which I quote from his Researches, 826, 828:—

"A single ion, i. e. one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly

indifferent to the passing current, unless it be itself a compound of more elementary ions, and so subject to actual decomposition."

"If, therefore, an ion pass towards one of the electrodes, another ion must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance."

81. In explanation of the mixed precipitates produced upon the membrane, I suggest that the hydrated oxide resulted from chemical reaction between the alkali and acid, the oxide from the oxygen of the water or potassa acting as a cathion in place of that of the oxide of copper: also that the metallic copper is to be attributed to the solutions acting both as conductors and as electrolytes; so that, at the membrane, two feeble electrodes were formed, which enabled a portion of the copper to be discharged without combining with an anion, and a portion of oxygen to be discharged without uniting with a cathion. In this explanation I am supported by the author's account of a well known experiment by Faraday, in which a solution of magnesia and water was made to act as electrodes at their surfaces respectively.

82. There can, I think, be no better proof that no reliance should be placed on the experiments with membranes, in this and other cases where the existence of compound radicals in acids is to be tested, than the error into which an investigator, so sagacious as my friend Professor Daniell, has been led, in explaining the complicated results.

83. The association of two electrolytes, and the chemical reaction between the potassa and acid, which is admitted to have evolved the hydrated oxide, seem rather to have created difficulties than to have removed them.

84. In this view of the subject, I am supported by the opinion of Faraday, as expressed in the following language:—

"When other metallic solutions are used, containing, for instance, peroxides, as that of copper combined with this or any decomposable acid, still more complicated results will be obtained, which, viewed as the direct results of electro-chemical action, will, in their proportions, present nothing but confusion; but will appear perfectly harmonious and simple, if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity."

85. I cannot conceive, that in any point of view the complicated and "*confused*" results of the experiment of Daniell with electrolytes separated by membranes, are rendered more intelli-

gible by supposing the existence of salt radicals. I cannot perceive that the idea that the anion in the sulphate is oxysulphion, makes the explanation more satisfactory than if we suppose it to be oxygen. Were a solution of copper subjected to electrolysis alone, if the oxide of copper were the primary object of the current, the result would be analogous to the case of sodium, excepting that the metal evolved at the cathode, not decomposing water, would appear in the metallic form. If water be the primary object of attack, the evolution of copper would be a secondary effect.

86. It is remarkable, that after I had written the preceding interpretation of Daniell's experiments, I met with the following deductions stated by Matteuchi, as the result of an arduous series of experiments, without any reference to those of Daniell above mentioned. It will be perceived that these deductions coincide perfectly with mine.

87. I subjoin a literal translation of the language of Matteuchi from the Annales de Chimie et de Physique, tome 74, 1840, page 110:—

"When salt, dissolved in water, is decomposed by the voltaic current, if the action of the current be confined to the salt, for each equivalent of water decomposed in the voltameter, there will be an equivalent of metal at the negative pole, and an equivalent of acid, plus an equivalent of oxygen, at the positive pole. The metal separated at the negative pole will be in the metallic state, or oxidized, according to its nature. If oxidized, an equivalent of hydrogen will be simultaneously disengaged by the chemical decomposition of water."

88. Thus it seems, that the appearance of acid and oxygen at the anode, and of alkali and hydrogen at the cathode, which has been considered as requiring the simultaneous decomposition of two electrolytes upon the heretofore received theory of salts, has, by Matteuchi, been found to be a result requiring the electrolysis of the metallic base only, and, consequently, to be perfectly reconcilable with that theory.

89. In fact I had, from the study of Faraday's Researches, taken up the impression, that the separate appearance of an acid and base, previously forming a salt, at the voltaic electrodes, was to be viewed as a secondary effect of the decomposition of the water or the base; so that acids and bases were never the direct objects of electrolytic transfer.

Of Liebig's "Principles," so called.

90. Under the head of the "theory of organic acids," in Liebig's Treatise on Organic Chemistry, we find the following allegations dignified by the name of principles. Manifestly they must tend to convey a false impression to the student, that hydrogen has a peculiar property of creating a capacity for saturation, instead of being only the measure of that capacity, as is actually true, and likewise that in this respect it differs from any other radical.

91. The allegations to which I refer are as follows, being a literal translation from the French copy of the Traité of Liebig, page 7:—

"The hydrated acids are combinations of one or more elements with hydrogen, in which the latter may be replaced wholly or in part by equivalents of metals."

"The capacity of saturation depends consequently on the quantity of hydrogen which can be replaced.

"The compound formed by the other elements being considered as a radical, it is evident that the composition of this radical can exercise no influence on the capacity of saturation.

"The capacity of saturation of these acids augments or diminishes in the same ratio as the quantity of hydrogen, not entering into the salt radical, augments or diminishes.

"If into the composition of the salt radical there should be introduced an undetermined quantity of any elements, without changing the quantity of hydrogen extraneous to the radical, the atomic weight of the acid would be augmented, but the capacity of saturation would remain the same."

92. As by the advocates of the existence of "*salt radicals*," hydrogen is considered as playing the part of a metallic radical, and must, therefore, as respects any relation between it and the capacity of saturation, be in the same predicament as any other electro-positive radical, I cannot conceive wherefore laws, which affect every other body of this kind, should be stated as if particularly associated with hydrogen.*

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* There is, in some respects, a coincidence so remarkable as to the part taken by Dr. Kane and myself, with respect to hydrogen, that I quote here the language which has been held by us respectively on this subject.

Treating of hydrogen, Dr. Kane uses the following words:—"It was at one time supposed that it shared with oxygen the power of generating acids; and as sulphur, chlorine, iodine, cyanogen, &c., formed one class by combining with oxygen, so they formed a second class, called hydracids, by entering into union with

93. Would not a more comprehensive and correct idea be presented by the following language?—

94. From any combination of an acid with a base, either the base or its radical may be replaced by any other radical or base, between which and the other elements present, there is a higher affinity. Of course from acids called hydrated, from their holding an atom of basic water, either this base, or its radical (hydrogen), may be replaced by any other competent base or radical.

95. The premises being manifestly fallacious, still more so is the subsequent allegation, that in consequence of the hydrated acids being compounds formed with hydrogen, their capacity of saturation *depends* on the quantity of this element which can be replaced.

96. Is not this an inversion of the obvious truth, that the quantity of hydrogen present is as the capacity of saturation; and that, of course, the quantity of any element which can be substituted for it, must be in equivalent proportion? Would not a student, from this, take up two erroneous ideas—first, that the capacity of saturation is conferred by the radical, and in the next place, that of all radicals, hydrogen alone can give such a capacity? Is it not plain, that the assertion here made by the celebrated author, would be true of any radical?

97. Passing over a sentence which has no bearing on the topic under discussion, in the fourth allegation we have a reiteration

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hydrogen. * * * In the year 1832 I proved this view to be incorrect, that all the properties of the compounds of hydrogen combined to show that it was an eminently electro-positive body, that it took place along with iron, manganese, and zinc. * * * These views have been still farther corroborated by the researches of Graham. * * * There rests now no doubt, in the minds of philosophical chemists, that hydrogen is a metal enormously volatile."

This justifies the following language held in my letter on the Berzelian nomenclature.

"I am of opinion that the employment of the word hydracid, as co-ordinate with oxacid, must tend to convey the erroneous idea, with which, in opposition to his own definition, the author seems to have been imbued, that hydrogen in the one class, plays the same part as oxygen in the other. But in reality, the former is eminently a combustible, and of course the radical, by his own definition."

So entirely have I concurred in considering hydrogen as an aëriform metal, that, for more than twenty years, I have, in my lectures, accounted for the amalgamation of mercury when electrolyzed in contact with sal ammoniac, by inferring ammonia to be a gaseous alloy of two metallic ingredients, hydrogen and nitrogen being both aëriform metals.

and expansion of the error of those by which it is preceded. We are informed that the "*capacity of saturation augments and diminishes with the quantity of hydrogen which can be replaced,*" which is again an inversion of the truth, that the quantity of hydrogen varying with the capacity, the quantity of any other radical, competent to replace it, must be in equivalent proportion.

98. Is not the concluding allegation a mere truism, by which we are informed, "that if any undetermined quantity of any element should be introduced into the composition of the radical, without changing the capacity (as measured by hydrogen), the capacity would be found the same when measured by any other radical?"

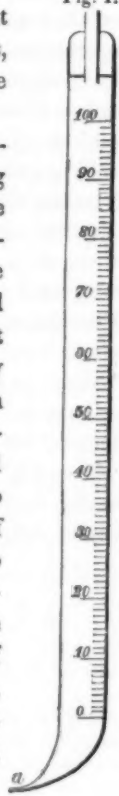
99. As all that is thus ascribed to hydrogen must be equally true of any other radical, there would have been less liability to misapprehension, had the generic term radical been employed wherever hydrogen is mentioned. But by employing the word radical to designate halogen elements, the advocates of the existence of compound radicals in amphide salts have deprived the word in question of much of its discriminating efficacy. In fact, their nomenclature would confound all ultimate elements under one generic appellation, and all their binary combinations under another, so that almost every chemical reagent, whether simple or compound, would be a salt or a radical.

100. Before concluding, I feel it to be due to the celebrated German chemist above mentioned, to add, that however I may differ from him as to the acids being hydrurets of compound radicals, I am fully disposed to make acknowledgments for the light thrown by his analytical researches on organic chemistry, and the successful effect of his ingenious theoretic speculations, in rendering that science more an object of study with physicians and agriculturists.

Art. IV.—*A New Instrument for estimating the quantity of Carbonate of Lime present in Calcareous Substances;* by J. Lawrence Smith, M. D.

Among the most ready methods used for the purpose of estimating the quantity of carbonate of lime contained in calcareous substances, are Davy's pneumatic and Rogers' methods, the one estimating it from the bulk of carbonic acid, and the other by the weight of the carbonic acid afforded by the action of an acid. The principal objection to the former, is the complication of the apparatus, and for the latter it is necessary to be furnished with a more than ordinary pair of balances, and a set of accurate weights, whereas the instrument about to be described is free from both these objections, with the additional advantage of affording more accurate results.

Fig. 1.



It appeared at first, that by taking a certain quantity of the substance to be examined, and letting fall upon it by degrees a solution of acid, the strength of which we know, that it might be possible to estimate the quantity of carbonate of lime in the same manner as the carbonates of the fixed alkalies are estimated; but for this to succeed, it is necessary that the substance should be finely pulverized, and free from any materials soluble in the acid used, but as it is not common to be furnished with these two conditions, another method had to be adopted, the principle of which is, to treat the calcareous substance with an *excess* of acid, the strength of which is known, and then to find out the amount of this excess, thereby knowing the quantity of acid taken up, from which we can easily calculate the quantity of carbonate of lime present. In the application of this principle, it will be found that any thing like difficult manipulation is avoided, and that there is no calculation required.

The first thing to be furnished with, is an instrument, which consists simply of a tube about half an inch in diameter, and ten inches long, having the principal part of it graduated in one hundred parts. The simplest form to be given to this tube, is such as is represented in figure 1, the extremity *a* being drawn out and bent downwards, leaving an opening so small as to allow a liquid to flow but slowly from the tube. To the upper part, for convenience' sake, is adapted a perforated cork, with a small tube; this is placed for the purpose of regulating the flow of the fluid, by placing upon it and withdrawing from it the finger, as we may wish to arrest or allow the liquid to flow from the extremity *a*. With this instrument, that I propose calling the *Calcarimeter* from its use, we must be furnished with two fluids, a solution of muriatic or nitric acid, and a solution of ammonia, both of which are prepared of a certain strength.*

Preparation of the acid solution.—This solution is prepared as follows: weigh out fifty grains of dry finely powdered pure carbonate of lime, or what is better, carbonate of lime precipitated from any of its solutions by carbonate of potash or soda; place this in a tea-cup or other convenient vessel, add to it about an ounce of water, (this is done simply for the purpose of moderating the action of the acid,) then take the muriatic or nitric acid of commerce, dilute it with one part of water; with this liquid fill the instrument to the 100 point, then let the acid fall gently upon the carbonate of lime, so as not to create a too great effervescence, and by proceeding carefully with the aid of a piece of litmus paper, we can find the exact point at which the carbonate of lime is all taken up, by the solution having an acid reaction. When we see that nearly all the lime is taken up we proceed very cautiously, by adding but a few drops of the acid at a time, and agitating the mixture considerably, for the purpose of bringing the insoluble carbonate well in contact with the different parts of the fluid. When the acid reaction commences, the acid is no longer added, and the point at which the acid now stands in the tube is marked, and by subtracting that from 100 we have

* The capacity of the instrument, from 0 to 100 is one ounce, and the length of the graduation had better be from eight to ten inches; of course this will vary with the diameter of the tube. As they are all to be of the same capacity, the graduation may be made upon the tube itself or upon a piece of paper and pasted on, then varnished, first with a solution of gum arabic, and afterwards with copal varnish.

the number of degrees of acid used to dissolve fifty grains of carbonate of lime, but as it is desired that the liquid should be so made as to require 50° of it to dissolve fifty grains of the carbonate, it is diluted with the proper quantity of water. For example, suppose the fluid marked 65° after the experiment; this indicates that 35° of the acid solution were required to dissolve the 50 grains. Now instead of 35° we require it to take 50° to dissolve the same quantity, so that by making up the difference between the thirty five and fifty with water the solution is prepared; that is to say, to every thirty five parts of the acid experimented with, fifteen parts of water are added. The solution can be again tested if necessary, and slight modifications made.

Preparation of the alkaline solution.—The alkaline solution is now prepared with ease. Let fall 50° of the acid into a vessel, then make a mixture of equal parts of ammonia and water, fill the instrument to the 100°, and let it flow upon the acid, and mark the point at which the acid is neutralized; suppose it to be twenty, then 80° have been used for that purpose; but it must be so made as that it will require 100°, therefore to every eighty parts of the solution experimented with, add twenty parts of water. In making either of these solutions, one gallon can be made with the same ease as one ounce, and moreover, when they are once made, there is never any necessity of recurring to the carbonate of lime, as the acid may now be prepared with aid of the ammonia.

Fig. 2.

Thus, then, 50° of acid dissolve exactly fifty grains of pure carbonate of lime, and 100° of the ammonia neutralize fifty of the acid.

As using the same tube for both acid and alkali is attended with some inconvenience, having to wash it out after using one before introducing the other, I have used an additional tube, (fig. 2,) about the same diameter and a little more than half as long as the calcarimeter, for the acid. It has simply three marks upon it; the capacity of the tube from the point marked *a* to the lower extremity is equal to the capacity of 50° of the other tube, and the other two marks correspond to ten and five; the use that is made of these, will be hereafter explained.

Manner of performing the analysis.—Being furnished with the two tubes, the two fluids, a cup or other convenient vessel, a small piece of glass rod a few inches long, a wine-glass, and a piece of litmus paper, a portion of which has been reddened by an acid, we proceed as follows. Weigh out fifty grains of the substance to be examined, place it in the cup and add to it about one ounce of water, fill the instrument last described up to the highest mark upon the stem with the *acid;* this is done by holding it between the thumb and fore-finger, having the little finger applied to the lower opening. After the acid is poured in, before withdrawing the finger, introduce the cork and place the fore-finger of the other hand upon the opening of the tube on the cork, for the purpose of preventing the liquid flowing out, when the lower opening is left unprotected; after seeing that the acid stands exactly at the mark, it is allowed to flow gradually upon the substance. After all the action has ceased, stirring it towards the end to insure this result, we fill the graduated tube with the solution of ammonia, in the same manner as we did the last, and let it fall gradually upon the mixture of acid and calcareous substance, arresting at will the progress of the flow, by simply placing the finger upon the tube in the cork. This instrument should always be transferred to the left hand and held in an inclined position. During the addition of ammonia, the mixture should be well agitated with the glass rod, and occasionally tested by bringing a little of it upon the extremity of the rod in contact with the litmus paper, and so soon as it ceases to turn this paper red, or begins to turn the red part of it blue, the experiment is completed, and we now look at what number of degrees the fluid stands in the tube and we are furnished with the per centage of carbonate of lime contained in the calcareous substance examined.* We may be saved the trouble of testing too often, by paying attention to the strength of the reaction of the fluid upon the litmus paper.

In most marls which have served as the subjects of my experiments, more or less alumina is to be found, a part of which is dissolved by the acid, of which part a very good use can be made. While adding the ammonia, the alumina immediately

* If magnesia happens to be present, it will be estimated as lime, but this will very seldom be a cause of error, as it exists very rarely in calcareous manures, for which this instrument is particularly intended.

around where the ammonia falls, is thrown out of solution, and if we stir the liquid, the alumina will be redissolved so long as there is any free acid, so that when the flocks of alumina are no longer taken up, we are furnished with an assurance that the process is nearly completed. The acid that the alumina and iron takes up is acted upon by the ammonia, with almost the same readiness as if free, so that no cause of error is to be apprehended from that source.

It may sometimes happen from oversight, that too much ammonia is added; notwithstanding this, the analysis need not be lost. Still holding the instrument in the left hand over the cup, having of course arrested the flow of the fluid, we pour some of the acid solution into the wine-glass, introduce the small end of the acid instrument into it, and allow it to rise on the inside to either of the small marks, and add this acid to the liquid and go on as before with the experiment, and at the conclusion read off what is indicated and to it add ten or twenty according as we may have added the acid measured by the first or second mark.

After what has been said, a few words will suffice to explain how the instrument operates.

It takes 50° of acid to dissolve fifty grains of carbonate of lime, or 1° to dissolve one grain, and it takes 2° of the ammonia solution to neutralize one of the acid, and therefore in treating a substance consisting in part of carbonate of lime, for every grain that is present one degree of the acid is taken up, so that when we come to add the ammonia, we know how much of the acid is taken up by the quantity of ammonia left behind, thereby knowing the number of grains of carbonate of lime, which we multiply by two, (as fifty grains of the substance was used,) to arrive at the per centage. This multiplication is not actually performed, as the instrument is so graduated as to dispense with it.

Were it at all necessary to give any evidence of its easy application, I might state that it, along with the fluid, has been placed in the hands of persons entirely unacquainted with chemistry, and even with the principle of the instrument, and they have, with some little instruction in the manipulations necessary, obtained results only one or two per cent. out the way, in their *first* examination.

ART. V.—*On the Method of Drs. Varrentrapp and Will for estimating the Nitrogen in Organic Compounds;* by J. LAWRENCE SMITH, M. D.

TO THE EDITORS.

As I sent you some time since, an account of a new method invented by MM. Varrentrapp and Will for estimating the quantity of nitrogen contained in organic substances,* it becomes my duty to see that you are furnished with an account of M. Reizet's investigation of this method, which investigations were presented to the Académie des Sciences in July of last year.

M. Reizet finds that the method is applicable, with some care, to substances containing large quantities of nitrogen, and small quantities of carbon, but when we analyze a substance poor in nitrogen and rich in carbon, the result is invariably inaccurate.

M. Reizet was led to suspect that there were errors attendant upon this method, from the fact that some years ago Mr. Faraday showed that organic substances containing no nitrogen, when heated with potash in contact with air afforded ammonia, a circumstance which must affect materially MM. Varrentrapp and Will's method, for they burn the organic substance, mized with potash and lime, in a tube, the tube being partly filled with air, so that when heated not only would all the nitrogen of the substance be converted into ammonia, but also the nitrogen of the air contained in the tube, so that when the nitrogen was calculated from this ammonia, a larger quantity than the substance rarely contained would be indicated.

Sugar and other substances devoid of nitrogen were burnt by M. Reizet, after the method made mention of, and he stated to me that he has obtained as much as 2 per cent. of nitrogen in burning 15 grains of sugar with the mixture of potash and lime, and the truth of which I have since witnessed. The following is a table of the results of some of his experiments upon sugar.

0.250	gram.	sugar	furnished	0.0038	gram.	nitrogen.
0.500	"	"	"	0.0075	"	"
1.000	"	"	"	0.0127	"	"
1.500	"	"	"	0.0149	"	"
2.000	"	"	"	0.0153	"	"

* See this Journal, Vol. XLII, p. 253, April, 1842.

Besides substances destitute of nitrogen, many that contained but a small quantity of nitrogen were experimented with, and always a larger quantity of ammonia was obtained than was due to the nitrogen present in the substance.

M. Reizet has also found that the mixture of alcohol and ether used, decomposes a small quantity of the bichloride of platinum, which is always added in excess, so as to form a small quantity of the protochloride, which being insoluble in this menstruum, is only a cause of error.

From these experiments, M. Reizet concludes that MM. Varrentrapp and Will's method should be used with great reserve, when new substances are the subjects of analysis, but he does not doubt that in the hands of skillful manipulators, it might become a valuable means of control.

The contents of this letter should have been communicated to you before, but I have been for some time intending to try a modification of this method, which suggested itself to me, as a means of remedying the defects pointed out by M. Reizet, but my occupations have prevented me from testing this modification, and as it is impossible for me to say when I shall be able to perform the necessary experiments, I have to content myself, in bringing it to your notice simply as a suggestion.

First, then, in preparing the mixture of potash and lime, after we have heated the mixture for the purpose of drying it, while hot, place it beneath a vessel containing oxygen gas, which gas will become condensed in the pores of the mixture, and prevent the nitrogen of the air finding its way in, when exposed to the atmosphere. Oxygen is preferred to hydrogen gas, from the fact that the former would be absorbed in larger quantity, and more effectually exclude the nitrogen. Thus much then for the preparation of the mixture. When employed, it is to be mixed with the substance to be analyzed, introduced into the tube, and the air of the tube is then to be driven out either by oxygen or hydrogen gas; if these precautions are taken, which will certainly not be found difficult of application, I doubt not that all error arising from the nitrogen of the air would be avoided.

Charleston, S. C., April 12, 1843.

Messrs. Editors—I have just received the January number of the Annalen der Chemie und Pharmacie, and there find an arti-

cle of M. Will's concerning M. Reizet's experiments upon the new method of estimating the nitrogen of organic bodies. M. Will has not been able to obtain an appreciable quantity of nitrogen from the combustion of sugar and other organic substances free from nitrogen.

1.214 gram. sugar candy, burnt with the soda and lime, gave 0.00086 gram. nitrogen, which represents 0.07 per cent. of the sugar used.

0.386 gram. stearic acid gave 0.00028 gram. nitrogen.

Numerous other experiments were made with a larger quantity of the same and other substances, with similar results, and he accounts for M. Reizet obtaining ammonia from sugar, by supposing that his mixture of soda and lime contained a nitrate, probably the nitrate of potash.

Dr. Fownes has also been testing the experiments of M. Reizet, and finds them incorrect, substantiating those of M. Will. 10 grs. crystallized tartaric acid gave him 0.127 p. c. nitrogen, a quantity too small to be considered.

M. Constantin Zwenger, in his article on Elaterin, (Ann. der Chem. und Phar. Sept. 1842,) states that this substance contains no nitrogen, having satisfied himself of that fact by MM. Varrentrapp and Will's method, the substance being constituted as follows, $C^{20}H^{14}O^{5}$.

I have also examined a specimen of animal charcoal that gave M. Laurent by the old method 2.5 per cent. of nitrogen, which when burnt with the soda and lime gave me 2.6 per cent.

Yours, &c. J. Lawrence Smith.

Charleston, June 9, 1843.

Art. VI.—*Remarks on the First Principles of the Differential Calculus, together with a new investigation of Taylor's Theorem;* by Prof. Theodore Strong.

Let φx denote any function of x, and suppose that x is changed to $x+h$, then φx becomes $\varphi(x+h)$, which it is our object to express in a series; considering x and h as indeterminate quantities, which are independent of each other.

We may evidently assume $\varphi(x+h)=\varphi x+(A+B)\psi h$, (1), (a finite expression;) and suppose that A is a function of x and in-

dependent of h, also that B is a function of x and h, such that it $=0$ when $h=0$, and that ψh is a function of h independent of x, and such that it $=0$, when $h=0$; for according to these suppositions when $h=0$, (1) becomes identically $\varphi x=\varphi x$, as it (evidently) ought to be.

Since h is arbitrary, we may put $2h$ for h in (1), and if we use B′ to denote the value of B when h is changed to $2h$, (so that B′ is the same function of x and $2h$ that B is of x and h,) (1) becomes $\varphi(x+2h)=\varphi x+(A+B')\psi(2h)$, (1′); also since x is arbitrary we may put $x+h$ for x in (1), and if we denote the increments of A and B (arising from the substitution of $x+h$ for x in A and B, which are supposed to be functions of x,) by ΔA and ΔB, it becomes $\varphi(x+2h)=\varphi(x+h)+(A+B)\psi h+(\Delta A+\Delta B)\psi h$, or substituting the value of $\varphi(x+h)$ from (1), $\varphi(x+2h)=\varphi x+(A+B)2\psi h+(\Delta A+\Delta B)\psi h$, (1″); and subtracting (1″) from (1′), we get $A[\psi(2h)-2\psi h]+B'\psi(2h)-2B\psi h-[\Delta A+\Delta B]\psi h=0$, which must be an identical equation; ∴ since A is independent of h, and B′, B, ΔA, ΔB, are not independent of it, (since each of them $=0$ when $h=0$,) we must have $\psi(2h)-2\psi h=0$ or $\psi(2h)=2\psi h$, and since h is indeterminate $\psi=1$, ∴ $2h=2h$, an identical equation, and h is arbitrary, as it ought to be; hence the equation is easily reduced to $2(B'-B)-\Delta A-\Delta B=0$, (2), which must be satisfied so as to be an identical equation.

Since $\psi=1$, (1) becomes $\varphi(x+h)=\varphi x+(A+B)h=\varphi x+Ah+Bh$, which shows that $Ah+Bh$ is the increment of φx arising from the substitution of $x+h$ for x, ∴ we may denote this increment by $\Delta\varphi x$, and shall have $\Delta\varphi x=Ah+Bh$, (3), so that (1) becomes $\varphi(x+h)=\varphi x+\Delta\varphi x$, (4).

First Principles of the Differential Calculus.

We may consider h as an increment of x, and denote it by Δx, and (3) becomes $\Delta\varphi x=A\Delta x+B\Delta x$, (3′), or $\frac{\Delta\varphi x}{\Delta x}=A+B$, (3″), which must manifestly be an identical equation, and be satisfied so as to leave Δx indeterminate; ∴ since A is independent of Δx, (or h,) the first member of the equation must be considered as having a term which is independent of Δx, ∴ if we denote this term by $\frac{d\varphi x}{dx}$, we get $\frac{d\varphi x}{dx}=A$, (4′), or $d\varphi x=\frac{d\varphi x}{dx}.dx=Adx$, (4″); where x is called the independent variable, φx a function of x,

and dx, $d\varphi x$ are called their differentials, and $\frac{d\varphi x}{dx} = A$ is called the differential co-efficient, since we must multiply dx by it to obtain $d\varphi x = \frac{d\varphi x}{dx} . dx = A dx$. The same results are readily obtained by writing for $\Delta\varphi x$ in the first member of (3′), $d\varphi x + \Delta'\varphi x = A\Delta x + B\Delta x$, the d relating to the term that involves the first power of Δx only, and the Δ' to the remaining part of the right member of (3′), $\therefore$ we get $d\varphi x = A\Delta x = A dx$, by using d for Δ, in the right member of the equation; this process shows the propriety of calling the method of obtaining the expression $d\varphi x = A dx$, (together with its various applications,) the differential calculus, since $A dx$ is only a part of the entire difference $Ah + Bh$, obtained by putting $d\varphi x = Ah = A dx$. We consider the method which we have given (deduced from considering (3′) or (3″) as an identical equation) for obtaining (4′) or (4″), as being the true foundation of the differential calculus.

These remarks however are to be understood as referring to the principles of the science; for in practice the common method of regarding $\frac{d\varphi x}{dx} = A$ as expressing the limit of the ratio $\frac{\Delta\varphi x}{\Delta x} = A + B$ when Δx is diminished in infinitum, is generally more simple and expeditious than any known method, and is therefore by no means to be abandoned.

Again, the method of Leibnitz, which consists in rejecting the term $B\Delta x$ in comparison with the term $A\Delta x$ in (3′) when Δx is indefinitely small, so that $\Delta\varphi x = A\Delta x$, or denoting these suppositions by using d instead of Δ, $d\varphi x = A dx$, has its practical advantages.

Finally, we may consider, (if we please,) $\frac{d\varphi x}{dx} = A$ as denoting the operation that must be performed on φx in order to obtain A, the co-efficient of the first power of h (only) in the expansion of $\varphi(x+h)$; for it is only this co-efficient that is obtained by the several methods that we have noticed; and we may observe that if we change h into dx, we shall get $\varphi(x+dx) = \varphi x + A dx + B dx$, and that the term $A dx$ is the differential of φx, so that we have $d\varphi x = A dx$, or $\frac{d\varphi x}{dx} = A$.

Investigation of Taylor's Theorem.

We shall now resume (2), $2(B'-B)-\Delta A-\Delta B=0$, (a), which we may put under the form $2(B'-B)-\Delta(A+B)=0$, (a'), which is to be satisfied so as to be an identical equation; hence since B' is the same function of x and $2h$, that B is of x and h, it is manifest that B must be of the same form as ΔA, also that B' has the same form as ΔA, excepting that we must use $2h$ in B' where we use h in ΔA or B.

If we substitute $\frac{d\varphi x}{dx}$ for A in (3) it becomes $\Delta\varphi x=\frac{d\varphi x}{dx}h+Bh$ $(3''')$, which substituted in (4) gives $\varphi(x+h)=\varphi x+\frac{d\varphi x}{dx}h+Bh$, (b); since A is a function of x, the form of ΔA must be similar to that of $\Delta\varphi x$ when we use A instead of φx, $\therefore$ we may put $\Delta A=\frac{dA}{dx}h+A'h$, (5), where $\frac{dA}{dx}$ is independent of h, and A' is a function of x and h, such that it $=0$ when $h=0$; since $A=\frac{d\varphi x}{dx}$ we get $\frac{dA}{dx}=\frac{d\left(\frac{d\varphi x}{dx}\right)}{dx}$, or (if we denote $\frac{d\left(\frac{d\varphi x}{dx}\right)}{dx}$ by $\frac{d^2\varphi x}{dx^2}$ as is customary,) $\frac{dA}{dx}=\frac{d^2\varphi x}{dx^2}$, which reduces (5) to $\Delta A=\frac{d^2\varphi x}{dx^2}h+A'h$, $(5')$; hence (from what has been said) we may represent B and B' by $B=B_{,}h+\theta(x,h).h$, $B'=B_{,}2h+\theta(x,2h).2h$, (6), where $B_{,}$ is supposed to be independent of h, and $\theta(x,h)$ denotes the same function of x and h, that $\theta(x,2h)$ does of x and $2h$, these functions being such as to $=0$ when $h=0$, so that the forms of B and B' are similar to that of ΔA, as they ought to be; and $\Delta B=h[\Delta B_{,}+\Delta\theta(x,h)]$, (7). By substituting the values of ΔA, B, B' and ΔB in (a), and rejecting the common factor h, it becomes after a slight reduction $2B_{,}-\frac{dA}{dx}-A'+4\theta(x,2h)-2\theta(x,h)-\Delta B_{,}-\Delta\theta(x,h)=0$, which is to be an identical equation, $\therefore$ since $2B_{,}$ and $\frac{dA}{dx}$ are independent of h, and since the other terms of the equation are not independent of it, (since each of them $=0$ when $h=0$,) we must put $2B_{,}-\frac{dA}{dx}=0$, or $B_{,}=\frac{1}{2}\frac{dA}{dx}=\frac{1}{2}\frac{d^2\varphi x}{dx^2}$,

$\therefore B=\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\theta(x,h).h$, which substituted in $(3''')$ gives $\Delta\varphi x=\frac{d\varphi x}{dx}h+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h^2+\theta(x,h).h^2$, (3^{IV}); and since the form of ΔA must be similar to that of $\Delta\varphi x$, when we use A instead of φx, we may denote ΔA by $\Delta A=\frac{dA}{dx}h+\frac{1}{2}\frac{d^2A}{dx^2}h^2+\theta'(x,h).h^2$, $(5'')$, $\theta'(x,h)$ denoting a function of x and h, such that it $=0$ when $h=0$; and if (according to the usual method) we denote $\frac{d\left(\frac{d^2\varphi x}{dx^2}\right)}{dx}$ by $\frac{d^3\varphi x}{dx^3}$, we get $\frac{d^2A}{dx^2}=\frac{d^3\varphi x}{dx^3}$; and we may here observe that we shall denote any expression of the form $\frac{d\left(\frac{d^{n-1}\varphi x}{dx^{n-1}}\right)}{dx}$ by $\frac{d^n\varphi x}{dx^n}$, where n is supposed to be a positive integer greater than zero. Instead of using the equation that remains after putting $2B_1-\frac{dA}{dx}=0$, we shall use (a') in what follows; and since the forms of B and B′ are to be similar to that of ΔA, we may by $(5'')$ represent them by $B=B_1h+B_2h^2+\theta''(x,h).h^2$, and $B'=B_1 2h+B_2(2h)^2+\theta''(x,2h).(2h)^2$, $\theta''(x,h)$ being the same function of x and h that $\theta''(x,2h)$ is of x and $2h$, each of these functions being $=0$ when $h=0$; and since $A=\frac{d\varphi}{dx}$, $B_1=\frac{1}{2}\frac{d^2\varphi x}{dx^2}$, we get $A+B=\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+B_2h^2+\theta''(x,h).h^2$; hence substituting the values of B, B′, and A+B in (a'), it becomes $2\left(\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+3B_2.h^2+\theta''(x,2h).(2h)^2-\theta''(x,h).h^2\right)-\Delta\left(\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+B_2h^2+\theta''(x,h).h^2\right)=0$, (a''').

If we develope $\Delta\left(\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\&c.\right)$ by $(5'')$, we get $\Delta\left(\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\&c.\right)=\frac{d^2\varphi x}{dx^2}h+\frac{d^3\varphi x}{dx^3}h^2+\&c.$ which being substituted in (a'''), rejecting the terms which destroy each other, dividing by h^2, then putting the terms which are independent

of h (or which do not $=0$ when $h=0$) equal to zero, we get $2.3B_2-\frac{d^3\varphi x}{dx^3}=0$, or $B_2=\frac{1}{2.3}\frac{d^3\varphi x}{dx^3}$; hence (3^{iv}) becomes $\Delta\varphi x=\frac{d\varphi x}{dx}h+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h^2+\frac{1}{2.3}\frac{d^3\varphi x}{dx^3}h^3+\theta''(x,h).h^3$, (3^v); and since ΔA has the same form as $\Delta\varphi x$, $(5'')$ becomes $\Delta A=\frac{dA}{dx}h+\frac{1}{2}\frac{d^2A}{dx^2}h^2+\frac{1}{2.3}\frac{d^3A}{dx^3}h^3+\theta'''(x,h)h^3$, $(5''')$, and as B and B′ must be of similar forms, it is evident from what has been done that we may put $B=B_1h+B_2h^2+B_3h^3+B_4h^4+$ &c., and $B'=B_1(2h)+B^2(2h)^2+B_3(2h)^3+B_4(2h)^4+$&c.; ∴ substituting these values and $A+B=\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2.3}\frac{d^3\varphi x}{dx^3}h^2+B_3h^3+$&c. in (a'), we get $2\left(\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2}\frac{d^3\varphi x}{dx^3}h^2+(2^3-1)B_3h^3+(2^4-1)B_4h^4+\text{&c.}\right)-\Delta\left(\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2.3}\frac{d^3\varphi x}{dx^3}h^2+B_3h^3+\text{&c.}\right)=0$, (a'^{v}), which is under a more convenient form than (a''').

If we develope $\Delta\left(\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2.3}\frac{d^3\varphi x}{dx^3}h^2+\text{&c.}\right)$ by $(5''')$, reject the terms which destroy each other, divide by h^3, then put the terms which are independent of h, equal to zero, we shall get $2(2^3-1)B_3-2\left(\frac{1}{2.3}+\frac{1}{2.4}\right)\frac{d^4\varphi x}{dx^4}=0$, or $B_3=\frac{1}{2.3.4}\frac{d^4\varphi x}{dx^4}$, and substituting this value of B_3 in (a'^{v}), we shall in the same way find $B_4=\frac{1}{2.3.4.5}\frac{d^5\varphi x}{dx^5}$, and so on; hence $A+B=\frac{d\varphi x}{dx}+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h+\frac{1}{2.3}\frac{d^3\varphi x}{dx^3}h^2+\frac{1}{2.3.4}\frac{d^4\varphi x}{dx^4}h^3+$ &c. and substituting this in $\varphi(x+h)=\varphi x+(A+B)h$, we get $\varphi(x+h)=\varphi x+\frac{d\varphi x}{dx}h+\frac{1}{2}\frac{d^2\varphi x}{dx^2}h^2+\frac{1}{2.3}\frac{d^3\varphi x}{dx^3}h^3+\frac{1}{2.3.4}\frac{d^4\varphi x}{dx^4}h^4+$ &c., (A), the law of continuation being evident.

(A) is the theorem of Taylor which we proposed to investigate; and we have obtained it without making any use of the binomial theorem. It may not be improper here to observe, that although (A) has been found on the supposition that x is indeterminate, yet it may be applied when a particu-

lar value is assigned to x, provided that none of the quantities φx, $\frac{d\varphi x}{dx}$, $\frac{d^2\varphi x}{dx^2}$, &c. becomes infinite when the particular value of x is substituted in them, and if any one of the aforesaid quantities is infinite, the true development will be given by (A) no further than to the term that first becomes infinite; and in order to obtain the development of the rest of the series, recourse must be had to particular processes of algebra. If we substitute the particular value $x=0$ in (A), and denote what φx, $\frac{d\varphi x}{dx}$, &c. become when we substitute $x=0$ in them, by $\varphi' x$, $\frac{d\varphi' x}{dx}$, &c., then if we write x for h, (which we may evidently do,) (A) will be changed to $\varphi(x)=\varphi' x+\frac{d\varphi' x}{dx}x+\frac{1}{2}\frac{d^2\varphi' x}{dx^2}x^2+$ &c., (A′), which is Maclaurin's theorem; and which is applicable always when the quantities $\varphi' x$, $\frac{d\varphi' x}{dx}$, &c. are none of them infinite; should any of these quantities be infinite, then we are to apply particular processes to obtain the development (when it is possible). If h is negative, the odd powers of h in (A) are negative, and must have the sign $-$; and similar remarks are applicable to (A′).

New Brunswick, April 21, 1843.

ART. VII.—*Notice of a portion of Dr. Dekay's Report on the Fishes of New York;* by D. HUMPHREYS STORER, M. D.

[Read before the Boston Society of Natural History, June 21, 1843.]

WHEN I read to this Society a few weeks since a notice of Dr. Dekay's Report on the Reptiles of New York, I promised to review his report on the fishes also of that state. Unavoidable circumstances have prevented me thus far from redeeming my pledge; but fearing I may still longer be detained from giving that careful attention to the volume referred to, which, from the munificent patronage under which it has been produced, will undoubtedly be looked upon as authority in the department upon which it treats, by scientific men abroad, if not in this country, I would at the present time offer you a few general remarks upon the descriptions of those species which are also found in the wa-

ters of Massachusetts. This I feel compelled to do, not merely to prevent the propagation of many errors which are observable throughout the pages before me, but also to enter my feeble protest against the efforts of any individual to stand on ground which is already occupied—to redescribe, under new generic and specific names, genera and species which have been known and acknowledged by scientific men.

Previous to presenting you these rough notes, I would remark, I have endeavored to persuade another, possessing more leisure and much more accurate knowledge of many of the fluviatile species contained in this volume than myself, to perform this duty, but he has refused to attempt it, and I have no alternative left me but to perform it myself. I cannot commence, however, without observing that I have left untouched the descriptions of the western and southern species, trusting the ichthyologists of those sections of our country will feel likewise called upon to point out any glaring defects which they may find to exist. I would only add, if at any future period I should ascertain that any of my observations now made are erroneous, I shall, upon this floor, correct them. Entertaining for Dr. Dekay no feelings save such as should actuate every naturalist, I shall be most happy, if I have unintentionally wronged him, to do him full justice—to prove to him, what I feel you already know, that my sole object is to establish the truth.

There is undoubtedly much valuable information in the volume before us; prepared as it is by one who in a manner represents the zoologists of the state of New York, it cannot be otherwise. It is not my purpose to dwell upon its merits; they will undoubtedly be fully appreciated by all who peruse it; I would merely point out to you some of its defects. And as I have been enabled to devote but fragments of time to its examination, I will follow the arrangement of our author.

On page 16, Dr. Dekay has thought proper to form a new genus, to contain a species which he calls *Pileoma semifasciatum;* but the description and figure of this fish place it in the genus *Etheostoma* of Rafinesque.

Boleosoma tessellatum, (p. 20.)—This species was described by me before this Society in April, 1841, and the description, accompanied by a figure, was published in the number of your Journal for January, 1842, under the name of *Etheostoma Olmstedi*, from

its discoverer, Mr. Charles H. Olmsted, of East Hartford, Conn. Dr. Dekay observes, "it approaches the genus Etheostoma in the form of its head, but its opercules are said not to be scaly." In this species the preoperculum is destitute of scales, although the artist in the figure before us has pretty liberally distributed them over both the gill-covers; and the lower portion of the operculum is naked. Such appearances would almost justify Rafinesque when he formed his genus, to say the "gill-covers without scales." The genus Perca is described as having bony opercula, and the genus Labrax differs from Perca in having scaled opercules; still the upper portion of the opercula of the Perches exhibit a greater or less number of well marked scales. If Dr. Dekay is dissatisfied with Rafinesque's genus, I am unwilling he should thus presumptuously attempt to expunge my specific name; personal friendship, as well as the most common rules of scientific etiquette, prompt me to act on the defensive.*

Pomotis appendix, (p. 32.)—Inasmuch as one of the characters of the genus Pomotis is "opercule with an elongated membrane at its angle," Mitchill's specific name of the fish here described is evidently inappropriate, and it will undoubtedly be changed by some future ichthyologist who shall have an opportunity of examining it.

Uranidea quiescens, (p. 61.)—About a year and a half since, Mr. Olmsted, of whom I have previously spoken, sent me a specimen of what appeared to him to be a new species. I at once recognized it to be the *Cottus viscosus* of Haldeman. That specimen is now in your cabinet. It is, in the volume before us, described as a new species under a new genus.†

Gasterosteus quadracus, (p. 67.)—Dr. Dekay in describing this species remarks, "Dr. Storer describes a membrane attached to the ventral spine, which escaped my notice." In a living specimen now swimming before me while I am writing this no-

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* Since these remarks were penned, Mr. Olmsted assures me that he examined in the state collection at Albany, with Prof. Emmons, the gentleman who discovered it, the identical specimen which Dr. Dekay describes in his report as the *Boleosoma tessellatum*, and that it is the same species that he sent me, and which I described two years since as the *Etheostoma Olmstedi*.

† Mr. Olmsted also states that the specimen which Dr. Dekay calls *Uranidea quiescens*, was examined by him in the same collection, in company with Mr. Haldeman, who unhesitatingly pronounced it to be his *Cottus viscosus*.

tice, a beautiful scarlet colored membrane is perfectly obvious, extending from the posterior edge of the ventral spine. In five specimens preserved in spirits, this membrane is equally distinct but colorless.

Rhombus triacanthus, (p. 137.)—The original description of this species by Peck is very accurate. When speaking of the situation of the spines from which it derives its name, he says: "There is a small horizontal spine, pointing forwards, at the beginning of the dorsal fin; another at the beginning of the anal fin; and a third, arising from the sternum and pointing backwards a little before the anus." Dr. Dekay describing the fins, observes: "Pectorals long and pointed. Anterior to this fin, is a broad acutely tipped movable spine; and before this, a broad axe-shaped movable plate or spine (see figure) occupying the place of the ventrals." This is unintelligible to us. A very minute spine at the origin of the anal fin, pointing forwards, and an equally minute spine situated in front of the anus, pointing backwards, are all we have ever been able to find upon the abdomen, although the figure illustrating this species exhibits three spines in this situation. All the specimens we have examined have had the edge of the gill-covers smooth; the superior anterior angle of the operculum in the figure referred to, shows two prominent sharp spines.

Gunnellus mucronatus, (p. 153.)—Under the description of *Muræenoides guttata*, Yarrell, in his "British Fishes," observes: "A specimen of a spotted gunnel from America, for which I am indebted to the kindness of Mr. Audubon, proves on comparison to be in every respect so similar to the British gunnel, that there is little doubt it is the same species." 2d. edit. Vol. I, p. 271. Dr. Dekay says of this species, "it resembles the G. vulgaris of Yarrell, but from the above description is evidently distinct from that species." I would only remark, that Yarrell, with specimens of both the foreign and native fishes before him, would be as likely to be correct as our countryman, who does not intimate that he ever saw a foreign specimen.

Lophius Americanus, (p. 162.)—Had our author told us he had compared our species with a specimen of the foreign fish Lophius piscatorius, and had he pointed out any differences which he had noticed to exist during that examination, we should undoubtedly have yielded our assent to his opinion; but until some

ichthyologist has compared the two, and shown distinctions, we shall continue to believe our species to be the old Lophius piscatorius of Linnæus, Cuvier, Pennant, Donovan, Fleming, and others.

Batrachus tau, (p. 168.)—We have here an instance of the absurdity of retaining the specific name first given to a species, whether it has any or no significancy—whether it is appropriate or not. I will use the words of Dr. Dekay: "The apparently odd specific name of *tau*, given by Linnæus, is derived from the Greek name of the letter T; such a figure being produced on the head by two elevated lines in the dried specimens." In other words, this species cannot be identified when living, because its scientific (not its natural) characters do not appear until it is dead.

Ctenolabrus uninotatus, (p. 174.)—This fish, which is made a new species by Cuvier, in which he is followed by Dekay, is a mere variety of the common burgall, which may be seen by an occasional visit to the fish market.

Pimelodus catus, (p. 182.)—It is very questionable whether this specific name should be retained, when there are several species of the genus, all of which our author calls cat-fish.

Labeo gibbosus, (p. 194.) *Catostomus tuberculatus*, (p. 199.) The former of these species I introduced into my report on the fishes of Massachusetts as a new species, upon the authority of Lesueur, with his description, never having met with a specimen myself. I was exceedingly surprised to find the two species in the volume before me classed under distinct genera. My friend Mr. W. O. Ayres, of East Hartford, Ct., whose zeal in the cause of ichthyology is equalled only by his accurate knowledge of the species in his vicinity, has determined that so far from being distinct genera, they are not even distinct species, but that they really are one and the same fish.

Stilbe chrysoleucas, (p. 204.)—Dr. Dekay has marked out what he considers a new genus, and calls it Stilbe, and as a synonym of the species under this genus, he places "Leuciscus chrysoleucas, New York shiner, Storer, Fishes of Massachusetts, p. 88." Either Dr. Dekay or myself, are in error. He says there "is a short spine before the dorsal fin, which is short. Anal fin long." The identical specimen which furnished my description, which prompted Dr. Dekay to quote it as a synonym, has belonged to the cabinet of this society, since the hour it was described; it is now on your table for examination. The anal fin,

Klein in his characteristics of the genus Leuciscus, calls "short." In Dr. Dekay's new genus it is "long;" the species which he describes has fourteen rays in its anal fin; the species before you has one ray less, thirteen. The difference of the length of the anal fin then, consists in the thickness of a single membranous ray. But it is said to have a "short spine before the dorsal fin;" that spine I did not notice in the description of this species in my report, because it does not exist in the specimen before you, nor have I been able to find a vestige of a spine in ten other specimens I have examined, since the publication of the volume before me. In all other respects, it is clearly a Leuciscus, and the Leuciscus chrysoleucas.

Fundulus fasciatus, (p. 216.)—This is an Hydrargyra of Lesueur. Respecting the imperfect elaboration of this genus, I must beg leave to dissent from Dr. Dekay.

Fistularia tabacaria, (p. 233.)—Our author while speaking of the spotted pipe-fish, tells us, "its geographic range is therefore from Brazil to the coast of New York, and probably still farther north; for Smith, in his History of Massachusetts, speaks of having seen two specimens of this fish from the coast of Martha's Vineyard, in 41° 30′ north latitude." I shall make no comments upon the work here referred to, but would refer Dr. Dekay to Silliman's Journal, Vol. xxxvi, for a general notice of it, while I add that Dr. Smith's collection of fishes was purchased several years since by this Society, and that in that collection, were the two specimens Dr. Dekay refers to, and that one of those specimens was described by me in my report, as the Fistularia serrata, and that that same specimen was sent by me to Dr. Dekay, and was by him also figured and described on p. 232, as the F. serrata.

Osmerus viridescens, (p. 243.)—In the synonyms of this species Dr. Dekay has arranged "the Smelt, O. viridescens, Storer, Mass. Rep. p. 108." In my report, I catalogued this species as the "Eperlanus Artedi," and gave as a reason for so doing that "Cuvier does not acknowledge this to be distinct from the European fish, and therefore Artedi's name has the priority." I have yet to learn that this species is distinct from the European smelt.

Baione fontinalis, (p. 244.)—Dr. Dekay has thought proper to form a new genus for what is unquestionably a young brook trout—*Salmo fontinalis.*

Clupea elongata, (p. 250.)—We could have overlooked Dr. Dekay's copying our description of the common herring of Massachusetts, without giving us credit for the same, had he not unfortunately transferred an error contained in said description into his pages. Instead of the eyes being "two diameters apart," the distance between the eyes is less than the diameter of the eye.

Alosa tyrannus, (p. 258.)—Our common alewive is here catalogued under Latrobe's specific name of *tyrannus*, although it is acknowledged to be "absurd and unmeaning," because Peck's prior name of *serrata* "is a mere name without any specific character, or clue to its identity." Why is not Peck's name as appropriate here as that of *Pimelodus catus?* The specific character is evidently indefinite in both—in neither more so, however, than the name of *tyrannus* in this species; and we are not a little surprised that the New York ichthyologist had not retained the very acceptable and appropriate name of Dr. Mitchill, *vernalis*, for this species.

Amia occidentalis, (p. 269.)—Why is not this species the Amia calva, described and figured by Kirtland in Vol. III, No. 41, of the Boston Journal of Natural History, published in November, 1840, as inhabiting Lake Erie? Its size is the same as that species; the number of its fin-rays differ but slightly. Dr. Dekay, it is true, while pointing out its characters, says "tail unspotted;" but then he afterwards acknowledged, when speaking of the colors—"I can say nothing, as I had only a dried specimen."

Lota inornata, (p. 283.)—I read a description of this species to this Society, April 21, 1841, under the name of Lota brosmiana; that description, accompanied by a figure, appeared in your Journal for January, 1842.

Brosmius vulgaris, (p. 289.)—Dr. Dekay was right in doubting the identity of the American cusk with the European species. I satisfied myself a long time since, that I had committed an error in my report. As our author has never seen a specimen of Lesueur's cusk, I would here point out its differences from the foreign species. It is of a more elongated form; its dorsal and anal fins are united to the caudal fin; its eyes are oblong, and there is an immense difference in the number of their fin-rays. Dr. Dekay seems to have misunderstood my account of the color of this species. He says "the cusk of Storer, is uniform dark slate." I described, as I here state, in my report, a specimen

twenty five inches in length, and weighing between three and four pounds. That small specimen was of an uniform dark slate color; but on the next page, I say "in a specimen weighing twenty pounds," (or in other words, in the more mature fish,) "the color is brown upon the back, with yellowish sides and white abdomen." This, the adult fish, you perceive is not of "an uniform dark slate."

Lumpus anglorum, (p. 305.)—In my account of the lump fish in my report, when speaking of the ridge in front of the dorsal fin, I remarked, "this ridge is formed of distinct rays, which are very visible in the dried specimen." Dr. Dekay, who must have had my report before him when he described that species, because he quotes me as having seen "one which weighed seventeen pounds," observes "the dorsal lump without any vestige of rays; at least, I found none in two which I examined." A dried specimen belonging to the cabinet of this Society lies before you, in which the rays are perfectly obvious. If Dr. Dekay had dissected a specimen, he would never have made such an error; eight rays are distinctly seen upon removing the flesh.

Anguilla tenuirostris, (p. 310.)—The description of this species is one of the most accurate in the volume before us; and had it borne its true specific name, it should have been left untouched. This species is Lesueur's "Muræna Bostoniensis," and I cannot see an effort made to erase it, without protesting against such a step. Dr. Dekay says "it may possibly be the Bostoniensis of Lesueur, as given in his brief sketch of the Murænidæ of the United States; but the description is too incomplete to enable me to determine it with certainty." How does it differ from the Bostoniensis? Lesueur's species is "above of a dark olivaceous brown, throat and abdomen grayish, region of the anus yellow ochre, towards the tail reddish." Dekay's species is "grayish olive above, yellowish beneath." Lesueur's, "jaws acute and short." Dekay's, "head small, tapering to the jaws." Lesueur's, "length about twenty four inches." Dekay's, "length one to two feet." The differences I cannot perceive; they cannot be pointed out. Dr. Dekay says, "I think it probable, but am not so certain, that the common eel of Massachusetts, noticed by Dr. Storer, may also be referred to this species." I would only remark, I never saw but one species of eel in the Boston market; that eel, Lesueur saw in the same market, and called it Bostoniensis; that

same eel, Dekay has admirably described nearly thirty years subsequently, as a new species.

Syngnathus fasciatus, (p. 319.)—This species I described and figured several years since in my report, under the name of *S. Peckianus*. Dr. Dekay says his species differs from mine in the following particulars: "The body of our species, in front of the dorsal fin, is heptangular; head and rostrum proportionally longer; the greatest depth of the rostrum scarcely exceeding twice the greatest depth of the head. The dorsal fin longer than the head, measured to the posterior part of the operculum." My description reads as follows—"on each side of the anterior portion of the body are three ridges, and one passes from the neck through the middle of the abdomen to the vent;" these seven ridges, I supposed made it heptangular. The length of the head differs much in its proportions to the whole length of the fish in this species, and cannot be relied upon. Thus in three specimens lying before me at this moment, one measuring eight inches, has the head one inch long; another measuring eight and a half inches, has the head precisely as long as the former; and a third seven inches long, has the head seven eighths of an inch long. As great a difference is noticeable in the depth of the rostrum. I also, in my report, state the dorsal fin to be longer than the head.

Syngnathus viridescens, (p. 321.)—In my report, I called this species *fuscus*. The difference between Dekay's fish and that which I described is this—in his specimen its color was "dark olive green above;" in mine, the "body was of an irregular dull brown color above." You can judge whether a mere shade of color constitutes a specific character.

Lactophrys camelinus, (p. 341.)—I do not refer to this page to say aught of the genus, which appears to me however to be altogether unnecessary, but merely to observe, I regret that any scientific man should allow himself to refer to a mere popular work, particularly if that work has been publicly pronounced, and proved to be, full of errors and unworthy the slightest confidence. I would again refer him to the notice of a History of Fishes of Massachusetts, contained in Silliman's Journal for 1839.

Spinax acanthias? (p. 359.)—After quoting this species with a query, upon my authority, Dr. Dekay observes, "I am almost inclined to suspect our species distinct from that of Europe." This remark would have seemed much less singular if he had

given any reasons for his suspicion. The line which he quotes from me thus in parenthesis, could have weighed nothing with him, ("under the lateral line, a series of white circular spots," Storer,) because this appearance exists also in the foreign fish. Thus Yarrell, in his description of this species, says, "the upper part of the head, body, and fins, slate gray; under parts yellowish white; *young specimens generally exhibit a few white spots.*" 2d edit. Vol. II, p. 526. I stated the specimen which I described to be "thirty four inches in length"—you perceive it was an immature fish.

Had not these remarks become already so protracted, I should have made a few observations upon the Scopelus Humboldtii, Ammodytes Americanus? Hippocampus Hudsonius? and others. I hope that some ichthyologist possessing the leisure, will furnish a more extended notice of the volume I have thus cursorily examined.

It is to be regretted that Dr. Dekay should have studiously neglected the labors of Rafinesque. With all his eccentricities, and want of method and frequent want of accuracy, there is much worthy of commendation in his "Ichthyologia Ohioensis" —much which should claim the grateful remembrance, at least, of American ichthyologists.

ART. VIII.—*On Greek Verbal Roots in English;* by Prof. J. W. GIBBS.

GREEK verbal roots are liable to various changes or modifications, which disguise them more or less to the English eye, and prevent the full appreciation of the meaning of many important terms. Among these are some employed in natural science.

Modern philologists have attempted with great labor to classify these changes or reduce them to general principles, and to give a philosophical account of their origin. We propose to give their results so far as the English language is concerned.

I. The following are euphonic processes, having for their object merely to relieve the organs of speech, or to please the ear.

1. The radical vowel *a* is sometimes changed into *e*. This is effected by *attenuation* or *precession* of vowel sound. See Prof. A. Crosby: Greek Gram. § 29. Examples will occur as we proceed.

2. The radical letters, particularly a vowel and liquid, are sometimes transposed; as, *drag*on for *drac*on, 'sharp-sighted,' from $\sqrt{darc}$,

'to see,' by transposition √*drac*; *tmesis*, 'a separation,' from √*tam*, 'to cut,' by transposition and precession of vowel √*tme*; em*blem*, 'something inlaid,' from √*bal*, 'to cast' or 'lay,' by transposition and precession of vowel √*ble*.

3. The last consonant of the root sometimes adapts or accommodates itself to the first consonant of the suffix; as, *crypt*, 'hidden,' from √*cryb*, 'to hide,' by accommodation √*cryp*; pro*lep*sis, 'anticipation,' from √*lab*, 'to take,' by precession of vowel and accommodation √*lep*; *aps*is, 'juncture,' from √*aph*, 'to join,' by accommodation √*ap*; *prac*tical, 'doing,' from √*prag*, 'to do,' by accommodation √*prac*; apo*plec*tic, 'striking down,' from √*plag*, 'to strike,' by precession of vowel and accommodation √*plec*; *hec*tic, 'habitual,' from √*hech*, 'to have,' by accommodation √*hec*; *dog*ma, 'an opinion,' from √*doc*, 'to seem,' by accommodation √*dog*; para*dig*m, 'an example,' from √*dic*, 'to show,' by accommodation √*dig*.

4. The last consonant of the root sometimes assimilates itself to the first letter of the suffix; as, *com*ma, 'a segment,' from √*cop*, 'to cut;' *lem*ma, 'a received truth,' from √*lab*, 'to take,' by precession of vowel √*leb*.

5. The last consonant of the root is sometimes cut off before the suffix by syncope; (1.) *n*; as, *cli*mate, 'a country in reference to its geographical position,' from √*clin*, 'to lean;' (2.) *d*; as, *phrase*, 'a speaking,' from √*phrad*, 'to say;' (3.) *th*; as, *plas*tic, 'forming,' from √*plath*, 'to form,' by dropping the final *th* and strengthening the vowel √*plas*; (4.) the digamma or *u*; as, *plei*ad, the name of a star, from √*pleu*, 'to sail,' by dropping the final *u* and then protracting the vowel *e*, √*plei*.

6. The final vowel of the root is sometimes strengthened before the suffix by an epenthetic *s*; as, *caus*tic, 'burning,' from √*cau*, 'to burn;' *schism*, 'a division,' from √*schid*, 'to divide,' by dropping the final *d* and strengthening the vowel √*schis*; *spasm*, 'a contraction,' from √*spa*, 'to draw.'

II. The following changes arise from internal inflection, or change of vowel within the root itself.

1. The radical vowel *a* is sometimes protracted by internal inflection; as, sy*ste*m, 'things standing together,' from √*sta*, 'to stand,' by protraction and precession of vowel *ste*. So em*blem* from √*bal*; *tme*sis from √*tam*.

2. The radical vowel *a* is sometimes changed into *o* by internal inflection; as, *ode*, 'a song,' from √*aed*, 'to sing;' *tome*, 'a volume,' from √*tam*, 'to cut;' *tone*, 'a note,' from √*tan*, 'to stretch;' para*bole*, 'a comparing,' from √*bal*, 'to cast' or 'lay.'

III. The following were originally emphatic processes for expressing with more force the idea of continued action.

1. The radical vowel *a* is sometimes protracted; as, *lem*ma, 'a received truth,' from √*lab*, 'to take,' by protraction and precession of vowel √*leb; phen*omenon, 'something appearing,' from √*pha*, 'to show,' by protraction and precession of vowel, and by the addition of *n*, √*phen*.

2. The radical vowel is sometimes strengthened by a nasal; as, *tymp*anum, 'a drum,' from √*typ*, 'to strike.'

3. The radical vowel is sometimes strengthened by guna, that is, *u* is changed into *eu;* as, *zeug*ma, 'a juncture,' from √*zyg* or *zug*, 'to join;' penta*teuch* from √*tych* or *tuch*.

4. The two first letters of the root are sometimes repeated; as, syn*agog*ue, 'an assembling together,' from √*ag*, 'to lead' or 'bring.'

5. The form of the root is sometimes lengthened, (1.) by the addition of a vowel; as, *æsthet*ic, 'pertaining to rhetoric or taste,' from √*aesth*, 'to perceive;' *Gene*sis, 'origin,' from √*gen*, 'to produce;' (2.) by the addition of the consonant *n*; as, dia*phan*ous, 'shining through,' from √*pha*, 'to show;' or *t*; as, *bapt*ize, 'to immerse,' from √*baph*, 'to immerse,' by accommodation and strengthening √*bapt;* (3.) by the addition of a vowel and consonant; as, *auxe*sis, 'increase,' from √*aug*, 'to increase.'

LIST OF GREEK VERBAL ROOTS IN ENGLISH.

1. √*a*, (Gr. √ἀ, =Sansc. √*wâ*,) breathing; whence *air* for *aer*, (√*a*+suff. *er*,) the fluid which we breathe; *aerial*, (√*a*+suff. *er*+suff. *i*+Lat. suff. *al*,) pertaining to the air.

2. √*aed*, (Gr. √ᾳδ,) by internal inflection *oed*, (Gr. ᾠδ,) singing; whence *ode*, (√*oed*+*e* mute,) a song; *tragedy*, (√*trag*+√*oed*+suff. *y*,) literally a goat-song.

3. √*aesth*, (Gr. √αισθ,) with lengthened form *aesthe*, (Gr. αισθε,) perceiving; whence *æsthetic*, (√*aesthe*+double suff. *tic*,) relating to perception, particularly of the beautiful.

4. √*aeth*, (Gr. √αιθ,) shining; whence *ether*, (√*aeth*+suff. *er*,) the shining upper air.

5. √*ag*, (Gr. √ἀγ, =Lat. √*ag*,) by internal inflection *og*, (Gr. ὠγ,) leading or bringing; whence *paragoge*, (pref. *para*+√*ag* repeated+suff. *e*,) a bringing or putting on of a letter or syllable to the end of a word; *synagogue*, (pref. *syn*+√*ag* repeated+*ue* mute,) a congregation of Jews; *demagogue*, (√*dem*+√*ag* repeated+*ue* mute,) a people-leader.

6. √*aph*, (Gr. √ἁφ or ἀφ, =Lat. √*ap*,) joining; whence *apsis*, plur. *apsides*, (√*aph*+suff. *sid*,) literally a juncture.

7. √*arch*, (Gr. √ἀρχ,) beginning, leading; whence *arch*, adj. chief; *archon*, (√*arch*+suff. *on*,) a Grecian magistrate; *monarch*, (√*mon*+

√*arch*,) one ruling alone; *archetype*, (√*arch* with union-vowel *e*+√*typ*+*e* mute,) first impressed, original; *architect*, (√*arch* with union-vowel *i*+√*tec*+suff. *t*,) a chief builder; *archduke*, (√*arch*+√*duc* with *e* mute,) a chief duke.

8. √*aug*, (Gr. √*αὐγ*, =Lat. √*aug*, Eng. *eke*,) with lengthened form *auxe*, (Gr. *αὐξε*,) increasing; whence *auxesis*, (√*auxe*+suff. *sis*,) increase, as the name of a rhetorical figure.

9. √*ba*, (Gr. √*βα*,) going; whence *basis* or *base*, (√*ba*+suff. *sis* or *se*,) a stepping, that on which any thing rests.

10. √*bal*, (Gr. √*βαλ*, =Lat. √*bal* in *balister*,) by internal inflection *bol*, (Gr. *βολ*,) by transposition and lengthening of the vowel *blē*, (Gr. *βλη*,) casting or laying; whence *symbol*, (pref. *syn*+√*bol*,) what by comparison suggests something else; *parabole*, (pref. *para*+√*bol*+suff. *e*,) a comparing or laying along side; *parable*, (the same form more fully Anglicized,) a species of extended comparison; *emblem*, (pref. *en*+√*ble*+suff. *m*,) something inlaid.

11. √*baph*, (Gr. √*βαφ*,) with final radical strengthened by *t*, *bapt*, (Gr. *βαπτ*,) dipping; whence *baptize*, (√*bapt*+suff. *ize*,) to administer the sacrament of baptism.

12. √*bo*, (Gr. √*βο*,) feeding; whence *botany*, (√*bo*+triple suff. *tany*,) the science of plants; *proboscis*, (pref. *pro*+√*bo*+suff. *sc*+suff. *is*,) literally what feeds before.

13. √*camp*, (Gr. √*καμπ*, = Sansc. √*kamp*,) with final radical strengthened by *t*, *campt*, (Gr. *καμπτ*,) bending; whence *anacamptic*, (pref. *ana*+√*campt*+suff. *ic*,) reflected.

14. √*cau*, (Gr. √*καυ*,) burning; whence *caustic*, (√*cau* strengthened by *s*+double suff. *tic*,) burning; *cautery*, (√*cau*+suff. *tery*,) an instrument for burning; *holocaust*, (√*hol* with union-vowel *o*+√*cau* strengthened by *s*+suff. *t*,) an offering which was wholly burnt.

15. √*chra*, (Gr. √*χρα*,) by lengthening the vowel *chre*, (Gr. *χρη*,) using; whence *catachresis*, (pref. *cata*+√*chre*+suff. *sis*,) abuse, as the name of a rhetorical figure.

16. √*chri*, (Gr. √*χρι*,) anointing; whence *chrism*, (√*chri* strengthened by *s*+suff. *m*,) unguent; *Christ*, (√*chri* strengthened by *s*+suff. *t*,) literally the anointed.

17. √*chro*, (Gr. *χρο*,) coloring; whence *chrome*, (√*chro*+suff. *me*,) a metal which affords beautiful colors.

Note.—The three preceding numbers, √*chra*, √*chri*, and √*chro*, are regarded as collateral roots, all signifying primarily *to touch the surface*.

18. √*chy*, (Gr. √*χυ*,) pouring; whence *parenchyma*, (pref. *para*+pref. *en*+√*chy*+suff. *ma*,) the spungy substance of the viscera.

19. √*cla*, (Gr. √*κλα*,) breaking; whence *iconoclast*, (√*icon* with union-vowel *o*+√*cla* strengthened by *s*+suff. *t*,) an image-breaker.

20. √*clin*, (Gr. √κλιν, =Lat. √*clin*, Eng. *lean*,) leaning; whence *clinic*, (√*clin*+suff. *ic*,) pertaining to a bed; *climate* or *clime*, (√*clin* +suff. *mate* or *me*,) a country in reference to its geographical position; *enclitic*, (pref. *en*+√*clin*+double suff. *tic*,) inclining.

21. √*cap* or *cop*, (Gr. √καπ or κοπ,) cutting; *capon*, (√*cap*+suff. *on*,) the name of a bird; *comma*, (√*cop*+suff. *ma*,) a segment; *apocope*, (pref. *apo*+√*cop*+suff. *e*,) a cutting off, as the name of a grammatical figure.

22. √*cra*, (Gr. √κρα,) mixing; whence *crasis*, (√*cra*+suff. *sis*,) a mixing, as the name of a grammatical figure.

23. √*cri*, (Gr. √κρι, =Sansc. √*kri*, Lat. √*kre* or *kri*,) sifting or separating; whence *crisis*, (√*cri*+suff. *sis*,) a separation, decision; *critic*, (√*cri*+double suff. *tic*,) pertaining to judging.

24. √*cryb*, (Gr. κρυβ,) with final radical strengthened by *t*, *crypt*, (Gr. κρυπτ,) hiding; whence *crypt*, (√*crypt*,) hidden, a vault; *apocrypha*, (pref. *apo*+√*cryb*+suff. *a*,) hidden, applied to books which were laid up at home and not read in public.

25. √*cy*, (Gr. √κυ,) containing; whence *cyst*, (√*cy* strengthened by *s*+suff. *t*,) a bag or tunic containing morbid matter in animal bodies; *cyma* or *cyme*, (√*cy*+suff. *ma* or *me*,) literally something contained.

26. √*darc*, (Gr. √δαρκ, =Sansc. √*dric*,) by transposition *drac*, (Gr. δρακ,) seeing; whence *dragon*, (√*drag* for *drac*+suff. *on*,) sharp-sighted, the name of an animal.

27. √*de*, (Gr. √δε,) binding; whence *anademe*, (pref. *ana*+√*de* +suff. *me*,) a chaplet of flowers; *diadem*, (pref. *dia*+√*de*+suff. *m*,) a head-band worn by kings.

28. √*dem*, (Gr. √δεμ,) by internal inflection *dom*, (Gr. δομ,) building; whence *dome*, (√*dom*+*e* mute,) a house.

29. √*dic*, (Gr. δεικ, =Sansc. *diç*, Lat. *dic*, Eng. *teach*,) showing; whence *paradigm*, (pref. *para*+√*dic*+suff. *m*,) an example; *apodictic*, (pref. *apo*+√*dic*+double suff. *tic*,) demonstrative.

30. √*do*, (Gr. √δο, =Sansc. *dâ*, Lat. *da*,) giving; whence *dose*, (√*do*+suff. *se*,) quantity given; *antidote*, (pref. *anti*+√*do*+suff. *te*,) a counteracting medicine; *apodosis*, (pref. *apo*+√*do*+suff. *sis*,) the application of a similitude.

31. √*doc*, (Gr. √δοκ, =Lat. *doc*,) thinking or seeming; whence *dogma*, (√*doc*+suff. *ma*,) an opinion.

32. √*dra*, (Gr. √δρα,) acting; whence *drama*, (√*dra*+suff. *ma*,) an action labored after the rules of art.

33. √*dram*, (Gr. √δραμ,) by internal inflection *drom*, (Gr. δρομ,) running; whence *dromedary*, (√*drom*+suff. *ad*+Lat. suff. *ary*,) a species of camel; *syndrome*, (pref. *syn*+√*drom*+*e* mute,) a concur-

rence; ***hippodrome***, (√*hipp* with union-vowel *o*+√*drom*+*e* mute,) a place for running horses.

34. √*ep*, (Gr. √*Fεπ*, =Sansc. √*watsh*, Lat. √*voc*,) saying; whence ***epic***, (√*ep*+suff. *ic*,) narrative.

35. √*erg*, (Gr. √*Fεργ*, = Eng. *work*,) by internal inflection *org*, (Gr. *Fοργ*,) working; whence ***organ***, (√*org*+suff. *an*,) an instrument; ***energy***, (pref. *en*+√*erg*+suff. *y*,) efficacy; ***liturgy***, (√*lit* with union-vowel *o*+√*erg*+suff. *y*,) public service.

36. √*eth*, (Gr. √*ἐθ*,) to be wont; whence ***ethic***, (√*eth*+suff. *ic*,) relating to morals.

37. √*gam*, (Gr. √*γαμ*,) marrying; whence ***polygamy***, (√*poly*+√*gam*+suff. *y*,) marriage with several.

38. √*gen*, (Gr. √*γεν*, =Sansc. *dzhan*, Lat. *gen* and *gna* or *na*,) by internal inflection *gon*, (Gr. *γον*,) and with lengthened form *gene*, (Gr. *γενε*,) producing; whence ***oxygen***, (√*oxy*+√*gen*,) acid-making; ***cosmogony***, (√*cosm* with union-vowel *o*+√*gon*+suff. *y*,) the origin of the world; ***Genesis***, (√*gene*+suff. *sis*,) origin, the name of the first book of Moses.

39. √*glyph*, (Gr. √*γλυφ*,) cutting in; whence ***glyph***, a cavity intended as an ornament; ***hieroglyph***, (√*ιερ* with union-vowel *o*+√*glyph*,) a sacred character.

40. √*gno*, (Gr. √*γνο*, =Sansc. √*dzhnâ*, Lat. √*gno* or *no*, Eng. *know*,) knowing; whence ***gnome***, (√*gno*+suff. *me*,) something known, a maxim; ***gnomon***, (√*gno*+suff. *mon*,) knowing, the style or pin of a dial; ***gnostic***, (√*gno* strengthened by *s*+double suff. *tic*,) knowing, belonging to a sect of oriental philosophers.

41. √*graph*, (Gr. √*γραφ*, = Lat. √*scrib*, Eng. *grave* and *scrape*,) digging; whence ***graphic***, (√*graph*+suff. *ic*,) descriptive; ***telegraph***, (√*tele*+√*graph*,) an instrument for communicating to a distance; ***hagiographa***, (√*hagi* with union-vowel *o*+√*graph*+suff. *a*,) sacred writings.

42. (Gr. √*ἁρπ*, =Lat. √*rap*,) seizing; whence ***harpy***, (√*harp*+suff. *y*,) a fabulous monster; ***harpoon***, (√*harp*+French suff. *oon*,) a harping-iron.

43. √*hech* or *sech*, (Gr. √*ἑχ* or *σεχ*,) by transposition *sche*, (Gr. *σχε*,) having; whence ***hectic***, (√*hech*+double suff. *tic*,) habitual; ***cachexy***, (√*cac*+√*hech*+suff. *sy*,) ill habit; ***scheme***, (√*sche*+suff. *me*,) a plan.

44. √*id*, (Gr. √*Fιδ*, =Sansc. √*wid*, Lat. √*vid*, Eng. *wit*,) with lengthened form *ide*, (Gr. *Fιδε*,) seeing; whence ***idea***, (√*ide*+suff. *a*,) an image.

45. √*lab*, (Gr. √*λαβ*, =Sansc. √*labh*,) by lengthening the radical vowel *leb*, (Gr. *ληβ*,) taking; whence ***astrolabe***, (√*astr* with union-

vowel *o*+√*lab*+*e* mute,) literally a star-taker; ***prolepsis***, (pref. *pro* +√*leb*+suff. *sis*,) anticipation; ***lemma***, (*leb*+suff. *ma*,) a received truth.

46. √*lamp*, (Gr. λαμπ,) shining; whence ***lamp***, a light made with oil and a wick.

47. √*leg*, (Gr. √λεγ, =Lat. √*leg*,) by internal inflection *log*, (Gr. λογ,) gathering, speaking; whence ***prolegomena***, (pref. *pro*+√*leg*+ suff. *omena*,) preliminary observations; ***lexicon***, (√*leg*+double suff. *sicon*,) a dictionary; ***dialogue***, (pref. *dia*+√*log*+*ue* mute,) a conversation.

48. √*lip*, (Gr. √λιπ, =Lat. *liqu*,) leaving, failing; whence ***ellipsis***, (pref. *en*+√*lip*+suff. *sis*,) an omission; ***eclipse***, (pref. *ec*+√*lip*+ suff. *se*,) literally a failure.

49. √*lit*, (Gr. √λιτ,) supplicating; whence ***litany***, (√*lit*+double suff. *any*,) a form of supplication.

50. √*ly*, (Gr. √λυ, =Lat. √*lu* in *solvo*,) loosing; whence ***analysis***, (pref. *ana*+√*ly*+suff. *sis*,) a resolving.

51. √*mach*, (Gr. √μαχ,) fighting; whence ***naumachy***, (√*nau*+ √*mach*+suff. *y*,) a fight of ships; ***monomachy***, (√*mon* with union-vowel *o*+√*mach*+suff. *y*,) a single fight.

52. √*man*, (Gr. √μαν,) to be mad; whence ***mania***, (√*man*+suff. *ia*,) madness.

53. √*math*, (Gr. √μαθ,) with lengthened form *mathe*, (Gr. μαθε,) learning; whence ***philomath***, (√*phil* with union-vowel *o*+√*math*,) a lover of learning; ***chrestomathy***, (√*chrest* with union-vowel *o*+√*math* +suff. *y*,) useful or necessary learning; ***mathematical***, (√*mathe*+ double suff. *matic*+Lat. suff. *al*,) pertaining to the science of quantity.

54. √*nem*, *nom*, (Gr. √νεμ, νομ,) pasturing, ruling; whence ***nomad***; ***antinomian***; ***astronomy***; ***economy***.

55. √*op*, (Gr. √ὀπ,) seeing, whence ***optic***; ***synopsis***; ***autopsy***.

56. √*path*, (Gr. √παθ, =Lat. √*pat*,) suffering; whence ***pathos***; ***pathic***; ***apathy***.

57. √*pau*, (Gr. √παυ,) ceasing; whence ***pause***.

58. √*pemp*, *pomp*, (Gr. √πεμπ, πομπ,) sending; whence ***pomp***, literally a sending under escort.

59. √*pen*, *pon*, (Gr. √πεν, πον, =Lat. √*pen* in *penury*,) laboring; whence ***geoponic***, laboring the earth.

60. √*pet*, (Gr. √πετ, =Sansc. √*pat*, Lat. √*pet*,) by syncope of the radical vowel and extension *pto*, (Gr. πτο,) falling; whence ***symptom***.

61. √*pet*, (Gr. √πετ, =Lat. √*pat*,) spreading out; whence ***petal***.

62. √*pha*, *phe*, (Gr. √φα, φη, =Lat. √*fa*,) speaking; whence ***prophet***; ***euphemism***.

63. √*pha*, (Gr. √φα, =Sansc. √*bhâ*,) with lengthened form *phan*, *phaen*, (Gr. φαν, φαιν,) appearing; whence *phase*; *diaphanous*; *phenomenon*.

64. √*pher*, *phor*, (Gr. √φερ, φορ, =Sansc. √*bhri*, Lat. √*fer*, Eng. *bear*,) bearing; whence *periphery*; *metaphor*; *phosphor*.

65. √*phil*, (Gr. √φιλ,) loving; whence *philter*; *philomel*.

66. √*phleg*, *phlog*, (Gr. √φλεγ, φλογ, =Sansc. √*bhrâdzh*, Lat. *flag*, and *fulg*, Eng. *blink*,) shining, burning; whence *phlegm*; *phlogiston*.

67. √*phrad*, (Gr. √φραδ,) saying; whence *phrase*; *periphrasis*; *paraphrase*.

68. √*phrag*, (Gr. √φραγ,) enclosing; whence *diaphragm*.

69. √*phtheg*, (Gr. √φθεγγ,) saying; whence *apophthegm*.

70. √*phthi*, (Gr. √φθι,) wasting away; whence *phthisis*.

71. √*phy*, (Gr. √φυ, =Sansc. √*bhû*, Lat. √*fu*, Eng. *be*,) being born; whence *physic*; *symphysis*; *neophyte*.

72. √*plac*, *ploc*, (Gr. √πλακ, πλοκ,) folding; whence *epiploce*, implication, a figure of rhetoric.

73. √*plag*, *pleg*, (Gr. √πλαγ, πληγ,) striking; whence *apoplexy*; *hemiplexy*.

74. √*plath*, (Gr. √πλαθ,) forming; whence *plastic*.

75. √*pleu*, (Gr. √πλευ,) sailing; whence *pleiad*.

76. √*pneu*, (Gr. √πνευ,) breathing; whence *pneumatic*; *pneumonic*.

77. √*po*, (Gr. √πο, =Sansc. *pâ*, Lat. √*po* in *potus*,) drinking; whence *symposium*.

78. √*poe*, (Gr. √ποι,) with lengthened form *poee*, (Gr. ποιε,) making; whence *poet*; *poem*; *epopee*.

79. √*prag*, (Gr. √πραγ,) doing; whence *pragmatic*; *praxis*.

80. √*pri*, (Gr. √πρι,) sawing; whence *prism*.

81. √*psa*, *pse*, (Gr. √ψα, ψη,) rubbing; whence *palimpsest*, an old parchment rubbed over or prepared anew.

82. √*psal*, (Gr. √ψαλ,) playing on an instrument; whence *psalm*; *psaltery*.

83. √*pty*, (Gr. √πτυ, =Lat. √*pitu* in *pituita*, Eng. *spit*,) spitting; whence *ptysmagogue*.

84. √*rheu*, *rhe*, (Gr. ῥευ, ῥε, =Sansc. √*sru*, Lat. √*ru* in *rivus*,) flowing; whence *rheum*; *rhetoric*; *diarrhea*; *catarrh*.

85. √*scad*, *scand*, (Gr. √σκαδ, σκανδ, =Sansc. √*scand*, Lat. √*scand*,) mounting; whence *scandal*.

86. √*scep*, *scop*, (Gr. √σκεπ, σκοπ, =Sansc. √*paç*, Lat. √*spec*, Eng. *spy*,) seeing; whence *scope*; *episcopal*; *bishop*.

87. √*schid*, (Gr. √σχιδ, =Sansc. √*tshhid*, Lat. √*scind*, Eng. *sheathe*,) dividing; whence *schism*.

88. √*spa*, (Gr. √σπα, =Lat. √*spa* in *spatium*,) drawing; whence *spasm.*

89. √*spar*, (Gr. σπαρ, =Sanse. √*sphar*, Lat. √*spar* in *spargo*,) scattering; whence *sperm.*

90. √*spend*, *spond*, (Gr. √σπενδ, σπονδ,) pouring out; whence *spondee.*

91. √*sta*, *ste*, (Gr. √στα, στη, =Sanse. √*sthâ*, Lat. *sta*, Eng. *stay*,) standing; whence *apostate; metastasis; apostasy; system.*

92. √*stal*, *stol*, (Gr. √σταλ, στολ, =Eng. *stall*,) placing, sending; whence *peristaltic; diastole; apostolic; apostle; epistle.*

93. √*steph*, (Gr. √στεφ,) crowning; whence *Stephen*, a proper name.

94. √*stig*, (Gr. √στιγ, =Lat. *stig* in *instigate*, Eng. *stick*,) marking; whence *stigma.*

95. √*streph*, *stroph*, (Gr. √στρεφ, στροφ,) turning; whence *strophe.*

96. √*tag*, (Gr. ταγ,) arranging; whence *tactic; syntax.*

97. √*tam*, *tom*, (Gr. √ταμ, τομ, =Lat. √*tem*,) by transposition and lengthening of radical vowel *tme*, (Gr. τμη,) cutting; whence *tome; atom; anatomy; epitome; tmesis.*

98. √*tan*, *ton*, (Gr. ταν, τον, =Sanse. √*tan*, Lat. √*ten*, Eng. *thin*,) stretching; whence *tone; tonic; hypotenuse.*

99. √*thaph*, (Gr. √θαφ, in θάπτω, =Sanse. √*tap*, Lat. √*tap*,) burying; whence *cenotaph.*

100. √*thraph*, *throph*, (√θραφ, θροφ,) nourishing; whence *atrophy.*

101. √*the*, (Gr. √θε, =Sanse. √*dhâ*, Lat. √*do* in *condo*,) placing; whence *thesis; theme; anathema; antithetic.*

102. √*thel*, (Gr. √θελ,) willing; whence *monothelite.*

103. √*ther*, (Gr. √θερ, =Lat. *ferv*,) to be warm; whence *thermal; anthracite.*

104. √*ti*, (Gr. √τι,) honoring; whence *Titus*, *Timon*, proper names.

105. √*trap*, *trop*, (Gr. √τραπ, τροπ,) turning; whence *trope.*

106. √*tych*, (Gr. √τυχ,) with guna of radical vowel *teuch*, (Gr. τευχ,) making; whence *pentateuch.*

107. √*typ*, (Gr. √τυπ, =Eng. *tap*,) striking; whence *type*, *tympanum.*

108. √*za*, *zo*, (Gr. √ζα, ζο, =Sanse. √*dzhiw*, Lat. √*viv*, Eng. *quick*,) living; whence *azote; zoology.*

109. √*ze*, *zy*, (Gr. √ζε, ζυ,) *boiling;* whence *apozem; zeolite; zumic; azyme.*

110. √*zo*, (Gr. √ζο, =Sanse. √*yu*, Lat. √*ju* in *jus*,) binding, girding; whence *zone.*

111. √*zyg*, (Gr. √ζυγ, =Sanse. √*yudzh*, Lat. √*jug*, Eng. *yoke*,) with guna of radical vowel *zeug*, (Gr. √ζευγ,) joining; whence *syzygy; zeugma.*

Art. IX.—*Remarks on Tides and the Prevailing Currents of the Ocean and Atmosphere;* by W. C. Redfield.

[Read before the American Philosophical Society at their centennial meeting, May 27th, 1843.]

The summary remarks and suggestions which follow, relate chiefly to the systematic currents of the ocean and the atmosphere; and were drawn up on short notice in the summer of 1838 at the request of a gentleman attached to the U. S. Exploring Expedition,* and were designed for reference, correction, and verification, by the scientific observers of the Expedition.

The views thus submitted I had derived, in previous years, from somewhat extensive examinations of the observations which had been made by voyagers and travellers in different seas and countries, and they are offered without an array of particular references to the numerous facts and observations from which they have been derived. This course was adopted, on that occasion, as being the least laborious, and because it was the undoubted design of the observers of the expedition to subject all general views and theories to the test of direct observations.

As a substitute, however, for those specific observations from which my results had been drawn, I delineated on maps and charts which were furnished me for the purpose, not only the general outlines or courses of the systems of general winds and currents which I had found to prevail in the Pacific Ocean and other seas, but also, some of the particular observations by which in my view, the existence of these currents had been established. These maps, seven in number, were lost by the unfortunate wreck of the Peacock, near the mouth of the Columbia River.

It is not my design to bestow further labor upon this extensive subject till the observations and results of the expedition shall have been published. But as observations on meteorology and the cognate branches of terrestrial physics may have been more limited in the expedition than I could have had reason to apprehend, particularly in the Atlantic, I venture now to lay before the Society my unfinished memoir of that period, even without those specific delineations which would have been afforded by the lost maps, which I have not yet attempted to reconstruct.

* James D. Dana Esq., geologist of the expedition.

I proceed now to the remarks which were addressed to the gentlemen of the expedition.

The preparation and departure of the Expedition fitted out by the goverment of the United States for the scientific examination of distant seas and countries, naturally awakens feelings of interest and expectation in the American public, as well as among the friends of science, in this and other countries. In such feelings the writer of these remarks fully participates, and the opportunities for useful observation which the Expedition is likely to afford, on various natural phenomena which have engaged his attention, may perhaps justify the following statements and suggestions, addressed to those who are to conduct the movements and perform the scientific labors of the expedition.

The instructions which have been drawn up by Sir J. F. W. Herschel, for observations in meteorology, and by M. Arago, for the discovery vessel, the *Bonite*, together with the valuable reports which have been made to the U. S. Naval Lyceum by its committee and other distinguished individuals, with direct reference to this expedition, have presented many important topics of investigation.* There are still, however, some points of interest and importance which seem to deserve more particular notice.

Indeed, the subjects of natural science which invite the investigation of the Expedition, are too numerous and important to be easily exhausted.

OF TIDES.

The valuable labors of Prof. Whewell and Mr. Lubbock have greatly enlarged our knowledge of the tides; owing chiefly to the fact that these gentlemen have followed the method of direct induction from actual observations, made at different localities. To the directions given by Prof. Whewell for obtaining the correct establishment, or true time of high water at the full and change of the moon, nothing more need be added.

It is a question of some importance, however, if it be not already determined, whether the main tidal wave of the North Atlantic be derived directly from the great Southern Ocean, as Prof. Whewell supposes, or, whether it mainly follows a circuit of revolution, north of the equator, around an elongated axis or

* See Naval Magazine for January, 1837, Vol II, p. 64, *et seq.*

neutral position, situated in mid ocean, somewhere between 18° and 26° north latitude, as had been suggested at an earlier period.*

A like question arises in regard to the tide-waves of both the North and South Pacific. The inquiry is therefore presented, whether the tidal wave in the North Pacific ocean does not move in a circuit, around a central position not greatly distant from the Sandwich Islands, the wave moving westerly in mid ocean in the intertropical or equatorial latitudes and easterly in the higher latitudes; and whether the tide-wave of the South Pacific does not follow a like course, around a central point or position at or near Tahita or the Society Islands. If this view of the course of the tide-waves should be sustained by observations in the Pacific, the tide-wave on the western coast of North America will be found moving southeastward, and together with the counter-wave from the South Pacific, might fully account for the extraordinary convergence and height of the tides in the Bay of Panama. Such a system of revolution in the tidal waves of the great oceans may account, also, for the absence of any considerable tides at the Sandwich and Society Islands, and at the Windward Islands of the Antilles.

Such circuits of revolution in the tides, would bear some analogy to those which, as I apprehend, are exhibited in the system of *currents* in the several oceans, as well as in the system of general winds, which likewise prevail. These systems of revolution and compensation, in the currents of the aqueous and aerial oceans, I have ventured to refer directly to the law of gravitation, as connected with unstable equilibrium and with the rotary and orbital movements of the several zones and meridians of the earth's surface.†

As connected with the enquiry on tides, it is important to ascertain the direction of the main stream of flood tide in the *offing*, at the several islands and prominent headlands which are most exempt from the local influences of reefs and shallows.

CURRENTS OF THE ATLANTIC.

The great system of aqueous circulation, which appears to be developed under various modifications in the several oceans on

* See note in Silliman's Journal for Oct. 1833, Vol xxv, p. 132.

† The law of equilibrium in our system, I apprehend, is a law of motion, not of rest.

both sides of the equator, has been glanced at in the foregoing remarks on tides. One of the most active, if not the best known current of this oceanic system, is the Gulf Stream of the North Atlantic. It appears to be established that a main portion of the Gulf Stream moves from the American coast towards the Azores and the Canary Islands, and thence along the coast of North Africa, turning westward till it again coincides with the equatorial current in its course towards the Caribbean Sea. This great circuit of the ocean current is found to coincide, mainly, with that which is also performed by the general winds in the basin of the North Atlantic. For the trade winds, on leaving the tropical latitudes, pass eastwardly through the temperate zone, but in a more irregular manner, sweeping around the track of ocean known as the *grassy sea* and the belt of summer calms, which lies a few degrees north of the tropic, known to navigators as the *horse latitudes*. It is in this extratropical region of calms that the major axis of this great elliptical circuit of general winds appears to lie. It is this calm region that separates the general westerly winds of the higher latitudes from the trade winds of which they are the counterpart; and it is chiefly these westerly winds of the higher latitudes which, in the performance of their great circuit of revolution, are again merged in the regular trade winds.* But let us return to the consideration of the more limited currents which prevail in the ocean.

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* I may add, that so far as the writer is concerned, the first exhibition of this view is found in my communication published in Silliman's Journal for April, 1831, Vol. xx, p. 50. In this instance, however, I have ascribed the currents of the ocean solely to the force of the winds, in compliance with the common theory; a view which I soon after found reason to abandon. The outlines of the great systems of horizontal revolution in the winds I have also sketched in my summary of "Facts in Meteorology," which appeared in Silliman's Journal for October, 1833, Vol. xxv, pp. 122–135. Previous to this period, I had examined the journals of whalers who had cruised on the "off shore ground" of the North Pacific, in that belt of calms and light winds near the latitude of 30°, which is the favorite resort of the sperm whales in that sea, and which corresponds to the so called "horse latitudes" of the Atlantic. From this and other like evidence I had arrived at the conclusions which I now maintain.

Sir John F. W. Herschel maintains the connection or continuity of the trades with the prevailing westerly winds of higher latitudes; and refers to the well reasoned explanations of Capt. Basil Hall, based on the common theory. He also adds an important suggestion on the velocity of winds which subside from a higher position in the atmosphere, and which may serve to explain the steady violence which sometimes pertains to westerly gales in the United States and on the North Atlantic.—*Treatise on Astronomy, section* 200 *and ante.*

Having noticed that portion of the Gulf Stream which, on passing the bank of Newfoundland, moves towards the Azores and the African coast, we will now follow that considerable portion of the Stream which is found to pass towards the western coast of the British islands and along the coast of Norway, till it enters the polar basin. From this frozen region it again emerges in the great polar current, covered with floating ice, which, skirting the coasts of Labrador and Newfoundland, falls in with the Gulf Stream at the southern extremity of the Grand Bank, and now becomes, mainly, a subaqueous current, the deeper portion of which can be traced only by its propelling effect on the deeply immersed icebergs, which it forces athwart the warm tropical stream, till they become dissolved by the higher temperature of the latter.

Observations of the temperature made in sounding at various depths in the Gulf Stream, and particularly in the region where it overruns or crosses the polar current, would be of high interest, and of great value in estimating the dynamics of the ocean currents.

As connected with the foregoing outline of the main system of superficial currents in the North Atlantic, I propose now a particular inquiry, relating to a single branch of this system of ocean streams, which perhaps may serve to show the origin or character of some currents which pursue opposite directions in other oceans. *From what source, then, is that southwesterly current derived which commonly prevails along the coast of the United States, in the direction which is opposite to the Gulf Stream?*

I am aware that this is usually considered by seamen as an eddy current, derived from the Gulf Stream; but from this view I am compelled to dissent. For, in the first place, this current never assumes the gyrating form of an eddy; but continues its course, when unobstructed by gales, in a direction which is generally parallel to the coast. But, secondly, in case this current be derived from the Gulf Stream, it must necessarily partake of the same elevated temperature; whereas, the reduction of temperature which occurs on crossing the northwestern limit of the Gulf Stream is most remarkable, and is almost without a parallel in the Atlantic, except in the immediate vicinity of ice.

It appears vain to allege the proximity of soundings or shallows as explaining this extraordinary change of temperature, for this cannot avail if the waters of the counter current be derived

from the Gulf Stream, to say nothing of the erroneous character of the position here noticed.

From the evidence which is afforded by numerous facts and observations, it appears that the current in question is neither more nor less than a more sluggish prolongation of the polar or Labrador current, which sweeps along the northeastern shores of this continent and the island of Newfoundland. And this current, if I mistake not, may be directly traced in its gradations of temperature, by the thermometer, from off the southern coasts of Newfoundland and Nova Scotia through the entire distance to Cape Hatteras, if not to Florida.

An eddy current offsetting from the Gulf Stream, would no where be so likely to be met with as at the point of intersection of this stream with the extremity of the Grand Bank of Newfoundland, and sweeping from thence upon the southern shores of the island of that name; and yet, the harbor of St. John's on the southern coast of Newfoundland is known to have continued ice-bound in 1831 so late as the month of June, although in the latitude of Paris. This fact is a convincing proof of the unimpeded continuation of the polar current to the southward, in this region, notwithstanding the near proximity of the Gulf Stream.

That Col. Jonathan Williams and others should have ascribed the reduced temperature of the ocean near our shores simply to the effect of shoals or shallow soundings, need not excite our surprise, as such striking reductions of temperature are found on the Great Bank of Newfoundland, and on that of the Lagullas, off the Cape of Good Hope, and while so little has been known of the system of ocean currents, and the proximate origin and courses of the colder streams of this system. And it is well known, that the low temperature of the sea on these banks and shallows has been ascribed to the effects of radiation. But, if I mistake not, it has been shown that a non-luminous body is incapable of radiation through water; and should this be otherwise, any possible effect of this kind is wholly overborne by the cold of the great polar currents, which constantly traverse the banks and shoals referred to.

If I am correct in this view, it is the reduced temperature of the currents from the polar regions, or, from contiguous ocean depths, which has led Williams, Davy and others to support the erroneous, or at least very questionable generalization, which as-

cribes a reduced temperature to the sea on all banks and shallows. If the ocean was devoid of currents, I think we might expect an *increase* of temperature on shoals in summer, or in warm latitudes, and a *reduction* of temperature in winter. A friend who made a full set of observations in crossing the Atlantic, informs me that on arriving at soundings in the English Channel, he found an increase of 2° in the temperature of the waters.*

Perhaps I may be allowed to refer, for a moment, to the geological agencies of the polar currents. It is well known that extensive fields and packs of ice, including many icebergs of vast magnitude, are constantly carried by the polar currents towards the lower latitudes. On reaching certain regions, such as the banks of Newfoundland and the Lagullas of Southern Africa, the ice is brought into proximity or contact with the warm counter-currents of the system, which flow from the torrid zone, where the ice is soon dissolved. The numerous masses of earth, rocks, beach bowlders, and sedimentary matter, which are borne by the ice in great profusion from the cliffs, the shores and the sea-bottom of the Arctic regions, and probably also from the Antarctic, are thus added continually to the vast submarine deposits which there accumulate. May not the continuance of this transporting process, through a long series of ages, be deemed sufficient to account for the existence and present extent of the great banks referred to; without particular reference to the evidence of successive elevations and subsidences, in extensive areas of the earth's crust?

CURRENTS OF THE SOUTHERN AND PACIFIC OCEANS.

That the currents of the Atlantic Ocean are connected with, and form an extension of those of the Indian and Southern oceans, has been proved by the researches of Rennel and others. Hence it follows, that the drain of these currents must be compensated by other currents which pass from the Atlantic to those seas, by some unknown or unexplored route, currents which move either at the surface or at lower depths. If these compensating currents exist at the surface, as is quite probable, on what meridians of the extreme South Atlantic are they to be found?†

* George W. Blunt, Esq.

† The consideration of the connection of the currents of the North Atlantic and the Arctic seas with those of the North Pacific, through Bhering's Strait, has been purposely omitted, as being less important in a general view, and beyond the expected range of observation by the expedition.

In view of an attempt to penetrate the Antarctic regions, it seems important to ascertain those routes by which the warmer currents of the great Southern ocean enter the polar basin, and on what routes or meridians they again emerge as ice-bearing currents, moving towards the lower latitudes. The thermometer will prove an important auxiliary in determining these localities, and the course of the polar currents from the Antarctic basin is now partially known, by the course of the icebergs which descend to the lower latitudes. It is by following the course of the warmer currents which enter the polar basin that the nearest approach will probably be made to the Antarctic pole; and the same system of continuous current might afford the means of final escape, should a ship be compelled to winter in the ice of that perilous region.

As regards the great system of currents in the Pacific, we may infer from the facts already known, that a current from the Antarctic region sets to the northward, several degrees west of Cape Horn, which unites its waters with those of the more temperate latitudes in their flow to the coasts of Chili and Peru, and thence towards the equator. If an ice current does not thus unite with that of the coast, the latter is mainly supported by the great afflux of the extratropical currents from the west, which, in performing their constant circuit of revolution, next sweep from the coast of Peru towards the equatorial latitudes, where they continue their course to the westward, again to leave the intertropical latitudes with an elevated temperature, which is in turn conveyed to the higher latitudes.*

The numerous archipelagos of islands and the extensive groups of coral reefs in the Pacific, serve to intercept the regular westerly progress of its warm intertropical currents, and to determine more than one circuit of compensation and revolution in each hemisphere. This class of obstructions partly supplies the place of a continent, in defining separate basins of revolution for the currents of this vast ocean, and this is particularly the case in the South Pacific, where these obstructions are scattered over wide areas. Hence, strong currents setting to the eastward have

* From information which I have gathered, I entertain no doubt of the blending of this ice current with the general current towards the equator on the west coast of South America; and the very reduced temperature which this current carries to the equator, at or near the Gallapagos Islands, is proof of the fact.

been found in various parts of the Pacific, below the latitude of 30°, moving in direct opposition to the influence of the strongest portion of the trade winds.* Thus the system of currents, as we shall find of the winds, becomes more complex and irregular in this vast ocean than in the Atlantic; which, at least so far as relates to winds, is contrary to representations which have been often erroneously made by scientific writers; representations which doubtless were founded in general reasonings on the calorific theory of winds.

Good observations on the direction, strength, and temperature of the currents, in all parts of the Pacific, will prove of great importance, and should be made and registered, most carefully, by the expedition.

The obstacles which thus modify the natural system of currents are least numerous in the North Pacific, where the trending of its continental coasts, except in high latitudes, is highly favorable to a strong development of the regular geographical currents, near to these coasts. Hence, on the coasts of China and Japan we find a current which fully represents the Gulf Stream of the Atlantic. This current, I find, was frequently noticed, incidentally, by the officers of Cook's last exploring expedition, and its velocity stated, in some instances, at five miles an hour. Other observations, to which I have had access, have confirmed the existence of this current, and have shown the elevated temperature which this stream carries from the lower latitudes; so that near one thousand miles east of the coast of Japan, in lat. 41° north, the temperature of the surface water has been found at 79½° of Fahrenheit.† In the South Pacific, near the coast of New Holland there is found, also, a like warm current, pursuing its southern circuit, through the higher latitudes of that hemisphere.

But owing as I apprehend, to the great width of the Pacific, and to the consequent absence of a defined ocean boundary near its central meridians, there is here less of apparent regularity and

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* This counter current, running to the eastward, is sometimes found in the equatorial regions of the Pacific and other seas, and bears some analogy to the westerly monsoons of the Indian and Pacific oceans.

† Voyage of Capt. Dupetit Thouars. Other and earlier observations had attracted my attention, particularly in the cruising voyages of our American whalers, but I now refer to this as a more recent and convenient authority.

system, both in currents and winds, than perhaps in any other ocean; the constant and reciprocal equatorial and polar tendencies of oscillation not permitting a single circuit of revolution to extend from Asia to America without deflection. Hence we find more apparent irregularity and complexity in the currents and winds of mid ocean, in this vast sea, than in those regions which are more nearly adjacent to the continental coasts.

A knowledge of the currents and winds of the Pacific Ocean, I am convinced, will serve to remove all mystery and all doubt from the once vexed question of the first peopling of its islands, from the Asiatic continent; in spite of the long urged objection of the opposition of the trade winds. A case is still recent where the wreck of a Japanese junk was drifted the entire distance to the Sandwich Islands, with its surviving crew; thus completing nearly half of the great circuit of winds and currents in the North Pacific. But we shall find an additional means of transport near the equator, which is afforded in the N. W. monsoon of the Indian and Pacific oceans, and which, according to my inquiries, is found to extend, at one portion of the year, as far eastward as the Society Islands; or more than half the distance from the Indian Ocean to the coast of South America.

OF GENERAL WINDS, OR PREVAILING CURRENTS OF THE ATMOSPHERE.

One of the most remarkable characteristics of the atmosphere is its constantly progressive action; exhibited in movements which are more or less rapid, and mainly horizontal.

To whatever general cause these movements may be ascribed, they are found in most countries to predominate in particular directions in the surface winds, but more uniformly at higher elevations. The greatest uniformity of the surface winds has been noticed chiefly in certain zones or regions which, for the most part, lie between the parallels of 30° latitude, north and south; limits which comprise half of the earth's surface. These more regular winds have hitherto been known best on the great routes of commerce, on the Atlantic and certain portions of the Indian oceans, and hence have been called the Trade winds.

In order to account for the supposed uniform character of the trade winds, a general theory of winds has been adopted, of much plausibility, founded on the alleged effects of calorific rarefaction

in the equatorial region. Aided by successive emendations, this theory continues to receive the general sanction of the scientific world.

It is not my design, in this communication, to discuss theories. But the facts and results which I have delineated on the accompanying maps,* indicate courses of circulation in the atmosphere which are nearly and mainly horizontal; while the common theory alleges a course or circuit of circulation, in each hemisphere, which is essentially vertical, the warm air being supposed to ascend near the equator to great elevations and there flow outwards, to supply the inward current from the higher latitudes; the obliquity from a north and south direction being of course due to the earth's rotation. I propose, therefore, to state in a summary way, some of the facts and considerations which, in my own view, serve to invalidate this calorific theory.

1. The specific difference of mean temperature in the intertropical winds as compared with equal zones of extratropical winds, is inadequate and wholly disproportioned to the dynamical effects which are exhibited in these winds. I am not aware that any successful attempt has been made to prove the converse of this objection.

2. The rising of the whole body of the trade wind in the equatorial latitudes, in the manner alleged, has never been confirmed by observation; and, as I apprehend, may safely be denied. Nor has any proof of the fact been offered, other than inferences drawn from common but very limited phenomena, which I think may be explained in a more satisfactory manner.

3. The perpetual snow line of the Andes has been found near one thousand feet higher in 16° to 18° south latitude than at the equator, or on the parallel of the equatorial calms of the Atlantic. This fact, in a region so favorable to an equable development of natural influences, I deem to be wholly conclusive against the theory.†

4. The semiannual change, to the north and south, of the locality of the trade winds and the belt of equatorial calms, which results from the change of seasons, bears no adequate proportion to

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* Those lost in the Peacock.

† See the observations of Mr. Pentland in the Journal of the London Geographical Society. Also, Penny Cyclopedia, Vol. VII, Art. CLIMATE.

the alternate geographical declination of the sun, nor to the actual geographical change in the zone of greatest temperature, which follows the sun's declination.*

The semiannual change of the locality in the trade winds is believed to be greatest in the Atlantic, where it does not appear to average more than 7° or 8° of latitude; while the annual range of the sun's declination exceeds 46°, and the actual transfer of the zone of heat, which follows the declination, appears to be nearly 40° of latitude. These facts, also, I deem to be conclusive against the theory.

5. Even within the ordinary geographical limits of the trade winds, there are extensive portions of the system of winds which, in their course and direction, do not accord with the received theory, but appear wholly irreconcilable with its requirements.

To illustrate this objection, I refer, first, to a circuit of intertropical winds in the equatorial basin of the North Atlantic, which appears to extend from the delta of the Quorra, the ancient Niger, for more than two thirds the distance to the coast of South America; in which circuit the winds revolve to the right, with more or less of regularity, around a central and probably elongated axis. And second, to the existence and great extension into open sea of those portions of the monsoon winds which blow obliquely *from* the equator, in directions where there can be none of the continental rarefaction which has been alleged as explaining these alternating winds. For if the winds of the equatorial latitudes rise to the higher regions, the monsoon winds of the Indian Ocean, on departing from the south side of the equator, could never be made to sweep eastwardly upon the earth's surface for even six thousand miles, as they now do annually, instead of ascending four or six miles in altitude, to flow off from the equator as superior winds.†

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* In other words, an essential geographical change in the locality of heat, of some months' duration, does not change, materially, the locality of the trade winds. Hence, these winds are not, mainly, the result of heat.

† M. Bougainville says, "from the 23d of February to the 3d of March, we had westerly winds, constantly varying between S. W. and N. W., with calms and rains; every day either a little before noon or soon after, we had sudden gusts of rain accompanied with thunder. It was strange to us to meet with this extraordinary wind under the tropic, and in that ocean so much renowned above all other seas for the uniformity and freshness of the E. and S. E. trade winds; which are

6. The sixth objection which I offer to the common theory of the trade winds, consists in the frequent occurrence, in our American climate, of the highest summer heats for several days in succession, sometimes irrespective of the immediate heat of the sun, which heated air, as appears from comparative observations, is mainly brought to us by geographical transfer along the earth's surface, and which appears to depart in the same manner. This could never happen if the most heated portions of the atmosphere necessarily ascend from the surface. A like objection is derived from the frequent interstratification and horizontal transfer of currents of unequal temperatures and hygrometrical conditions, which appear to move over great distances without any obvious change in their relative altitudes.

Having already noticed, in the course of these remarks, the system of horizontal circuits of revolution pursued by the winds on each side of the equator, it is now only necessary that I refer the observers of the expedition to the particular delineations of these circuits, and of the alternating system of monsoon winds, on the maps which are furnished herewith.*

It must not be supposed, however, that these circuits of revolution in the great winds, are generally uniform or strictly defined in their location or development, even on the open ocean. On the contrary, the winds which proceed outward from the trades, often overlie those which at the same time are returning into the trades. This often occurs extensively, on different meridians along the same parallel; besides the incidental fluctuations and disturbances to which the winds are always liable, and the shifting of their field of revolution to the north or south, by the change of seasons. But the general result, is a continued and

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said to reign in it all the year round. We shall find more than one opportunity to make the same observation."

This relates to the southern Pacific in long. 110° to 115° west from Greenwich, and serves to show an extension of the westerly monsoon winds at that season, even to the meridian of California. Numerous observations have tended to confirm the vast extension of these winds in the intertropical latitudes of the Pacific, opposite to the alleged course of the trade winds. Over the whole western coasts of intertropical America, the course of the winds is also at variance with the calorific theory.

* In the absence of the maps referred to, some general notion of the system of monsoon winds thereon delineated, may perhaps be obtained by referring to the summary description of these winds found in this Journal for October, 1833, Vol. xxv, p. 124–125.

successive series of laminated or stratified currents, overlapping and moving upon each other in like series of subordinate circuits, the major axes of which, in the northern summer, are principally found in the calms of the horse latitudes.

The calms and light winds which are peculiar to this last mentioned region in summer, result not so much from any general suspension of the aerial movements, as from the absence of that brisk relative motion which commonly prevails in other latitudes. For, the predominating movements of the atmosphere being either from the east or west, in conformity with the law of the earth's rotation, and there being little movement of the surface winds in these directions along the parallels in which lie the axes of atmospheric revolution, it follows, that only the more sluggish northerly and southerly winds chiefly prevail on these parallels, in mid ocean, at this season. And I may here suggest, that a like explanation is mainly applicable to the calms of the equatorial region, both between the regular trades, and the Indian monsoons.

Towards the eastern borders of a basin of revolution, such as the North Atlantic, there appears to be less of sluggishness in the aerial currents which move to or from the lower latitudes; which here appear more clearly defined and more strongly developed, and hence are more readily traced in their course; as is seen in the northerly winds which gradually merge in the N. E. trades, in the region between Madeira and the Canaries, and thence to the tropic. While, near the western borders of the Atlantic and over the adjacent coasts of America, the opposite southerly and southwesterly winds of the circuit are often well developed at the earth's surface, at least in the warm season. Like characteristics pertain to the system of winds in like latitudes, in other circuits of atmospheric revolution, in different oceans.

That the N. E. trade winds have not sooner been traced in their horizontal curves into the southwest winds, may be owing in part to the frequent overlying of the southwesterly upon the easterly winds, which occurs mostly towards the exterior portion of the trades; and partly, to a neglect to inquire into the actual and successively varying directions of the trade winds, in the central and western parts of the ocean basins, in the intertropical latitudes. In these latitudes, in the regions here mentioned, the N. E. trade winds are more often found nearly at east,

and veering to E. S. E. or S. E., than has been generally imagined.

But the courses traversed by storms, in the trade-wind latitudes of the western Atlantic, and in corresponding latitudes in the western portions of other seas, as shown by my own inquiries and those of Col. Reid, I conceive to have proved this horizontal course of atmospheric circulation, in the clearest manner; and it was this kind of evidence which first brought conviction to my own mind.* In pursuing this branch of the evidence we are thus able fully to establish the western half of the north Atlantic circuit of revolution in the general winds; while, the better defined courses of the regular winds from the latitude of Madeira to the trades, in the eastern Atlantic, is such as to remove all reasonable doubt of a nearly continuous circuit of revolution, from left to right, around the region of extratropical calms, called the horse latitudes.

I may add, on this occasion, that if further proofs were wanting of this horizontal circuit of revolution in these general winds, it is found in the *rotation* of the great storms, from right to left in the northern hemisphere, around their several moving axes, while pursuing their natural course of progression in this great aerial circuit. The question has often been asked, why should all these storms revolve in this direction, rather than in the opposite? And why the contrary rotation which is noticed in the southern hemisphere? Now I have been convinced for several years, that this law results from the conditions which necessarily attend the earth's rotation. For, in the westwardly movements of the atmosphere upon the earth's surface, obliquely from the equator towards the poles, the narrowing of the meridional spaces and the reduced velocities of rotation in the earth's crust on the parallels newly arrived at by the surface wind, with the constant retardations of eastern movement in the front of the mass which results

* See my published maps of 1830 and 1835 containing the tracks of storms; also, my communications in Silliman's Journal and the Nautical and Naval Magizines, since April, 1831; likewise, the charts &c., of Col. Reid, R. E., published in the Professional Papers of the Royal Engineers, and his elaborate work on the Law of Storms, issued at the time these remarks were in preparation; a copy of which work was received and forwarded to the expedition. More recently the labors of Mr. Piddington of Calcutta have afforded much additional evidence, as relates to the Indian and China seas.

therefrom, conjoin to induce a rotary tendency in the incumbent winds, in the very direction in which the storms are found to revolve.*

This dynamical tendency to gyration in the atmospheric currents or winds which are in contact with the earth's surface, is constantly productive of sensible effects, particularly as we proceed from the intertropical to the higher latitudes. This, I apprehend, is the chief cause of the changes and variableness of the winds in these latitudes, and also of the remarkable increase of the barometrical oscillations, the great storms being only the more strongly marked cases of gyratory action; while the numerous weaker or abortive cases which go to fill up the intervals of space, and partly overlie each other, and which are also modified by the ordinary disturbances of temperature and locality, have excited little notice or inquiry. It is this law of terrestrial rotation which, as I apprehend, is maintained by Prof. Dové of Prussia, in his attempts to show the elements of gyration in the general winds; a writer with whose labors I have been but lately and partially acquainted.

The general correctness of the foregoing view of the prime cause of local gyrations in the atmosphere, as well as the rotation in great storms, may be shown by an experiment made on the surface of a common globe; which I have occasionally pointed out to friends interested in these inquiries. Let a concave surface of wood or other substance, of a circular form and a diameter equal to five or ten degrees of the globe, be prepared and perforated with a small hole in the center, through which a pin may be loosely placed, to serve as an axis. Then let the concavity be lined with flannel or other yielding material, and placed upon the top of the globe near the equator. Then cause the globe to revolve from west to east in the direction of the earth's rotation, while the concave body is guided, carefully, by the pin at the axis, in the direction of the storm tracks which are found on my

* It will be noticed that the rotation of the great storms, as well as more ordinary atmospheric gyrations, is opposite to that of the great natural circuit of winds in which they are carried forward. Thus, if a general current of revolution swept around such a lake as Ontario from left to right, the eddies and local gyrations near its borders and in the body of the stream would exhibit a contrary rotation, from right to left.

chart of 1835,* and so as to impinge with equal weight and surface on all sides of the pin or axis, and the incumbent body will be found to revolve from right to left, in the manner of the storms of the northern hemisphere.

This experiment requires delicate management, and is more difficult because of the necessary rigidity of the incumbent surface, causing one part partially to counteract another; but in the case of a fluid, where all the particles move freely upon each other, no such impediment exists.

As it is chiefly the lower stratum of wind which is thrown into gyration from this cause, it must be evident, as above suggested, that within the geographical limits of the trade winds the great circuit of aerial revolution must be a nearly horizontal one, and that the storm tracks mark distinctly the usual course of this revolution. Consequently, the main outflowing course of the trade wind from the equatorial latitudes is not in the upper regions of the atmosphere.†

It was my design to have followed these general remarks with a detailed explanation of the delineations of the several systems of prevailing winds which I have placed on the maps before referred to. This was particularly my intention as relates to the extensive developments of the monsoons, and the several belts of light winds and calms which may be viewed as the anticlinal and synclinal axes, so to speak, of the several systems of general winds. But the lateness of the call and my necessary avocations have prevented me from fulfilling this labor, in time for the expedition.

This imperfect summary of the results of inquiries which I have pursued with no little interest, is now commended to the gentlemen of the expedition for their impartial examination; and with the expectation, and desire, that truth only, as apart from any favored theories, will be the object of their researches in natural science.

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* This chart may be found in this Journal, Vol. xxxi, for October, 1836; also in London Nautical Magazine, April, 1836, and Col. Reid's work on the Law of Storms.

† This is also shown by the extraordinary heat of the summers in countries near the western boundaries of the great oceans, this heat being conveyed horizontally by the surface winds from the lower latitudes; while in winter the results are modified and an opposite state of temperature induced, by causes which are peculiar to continental meteorology.

Art. X.—*Abstract of the Proceedings of the Fourth Session of the Association of American Geologists and Naturalists.*

(Concluded from page 165.)

Saturday, April 29, 1843.—After the reading of the minutes, *Mr. Dana* read a paper on the distribution of corals, alluding to a former statement of his, (p. 145) that the reef-forming corals are limited in their distribution in our present seas by the temperature of 66° Fah., and also to the fact of the absence of corals from the Gallapagos under the equator, and their presence at the Bermudas, added farther, that owing to these currents the coral zone limited by this temperature was singularly contracted on the western shores of the continents by the extratropical currents, and expanded on the eastern by the intratropical currents. On the western shores of America it was reduced to 16° of latitude in width, and in those of Africa to at least 12°, while in the mid ocean the zone is fully 56° wide, and on the eastern coast of Asia and New Holland 64°. These facts with regard to the influence of oceanic currents, may explain many anomalies in the distribution of fossils. Mr. D. denied the truth of Mr. Darwin's principle, that islands with barrier reefs are subsiding, and those with fringing reefs rising, and stated that he was not satisfied by his observations, that any elevations or subsidences were *now* in gradual progress in the Pacific or on the South American coast. His observations fully confirmed Mr. Darwin's theory, that *atolls* or coral islands were once barrier reefs around high islands, which high islands have disappeared by subsidence. From the distribution of these islands, he drew conclusions different from those of Mr. Darwin, with regard to the areas of subsidence. If a line were drawn in an E. S. E. direction from New Ireland by the Navigator and Society Islands, the islands to the north, with two or three exceptions, would be found to be low and coral, and those to the south, high and basaltic.* These coral islands diminish in number and size as we recede to the north from this line, and over a large area between the Sandwich Islands and the equator there is an open ocean without islands. As we go south from the same line, the extent of the reefs around the high islands diminishes. From these facts he concluded that the subsidence in progress during the formation of these islands was greater under the equator where the coral islands were few and small, than farther south, where they are numerous and large: that still greater subsidence took place over the open seas where the currents were too

* See the map accompanying Mr. Dana's article, published in the present volume of this Journal, page 131.

rapid to permit the growth of the coral to keep the islands at the surface: that the subsidence south of this line, where high islands prevail, was less than on the line, and still less than to the north of it. The large area of subsidence thus indicated, is not less than five thousand miles long and three thousand wide, and covers at least fifteen millions of square miles. He also alluded to the singular fact that the longer diameter of this area of subsidence corresponded to the trend of the Navigator, Society and Sandwich Island groups, and the low or coral archipelago.

As present opinions seem to require a balance motion in changes of elevation, he suggested that inasmuch as the tertiary rocks of the Andes and North America indicate great elevations since their deposition, that possibly during this great Pacific subsidence, America, the other scale in the balance, was in part undergoing as great or greater elevation. The absence of corals from the western tropical parts of this continent, was explained by reference to the extratropical currents before alluded to.

The President enquired of Mr. D. if he considered this subsidence to have taken place *equally* over the entire area.

Mr. D. replied, that it could hardly be expected that this effect should have been *exactly* uniform, considering the vastness of the area, but that considered as a whole it must have been nearly uniform.

The President objected to the supposition of Mr. Dana, that this vast amount of subsidence was due to the gradual refrigeration of the earth from a heated state; he thought it susceptible of mathematical demonstration that the amount of subsidence was too slight to account for the changes which must have taken place in the Pacific. Indeed, the earth must have acquired a nearly statical equilibrium of temperature before the existence of the corals, which probably did not make their appearance until the post-tertiary period.

Mr. Redfield said he had been much gratified with Mr. Dana's references to the fixed memorials of the warm currents in the Pacific, as afforded in the coral formations. Previous to the sailing of the Exploring Expedition he had the satisfaction of reviewing with Mr. Dana, partly in reference to a great dynamical question which regards the currents of the atmosphere and the ocean, some of the evidence which he, Mr. R. had before obtained of the character and courses of the great system of currents in the Pacific, and of the low temperature of the extratropical currents which sweep towards the equator along the western coasts

of North and South America.* The effect of these cold currents was such that the temperature of the sea at the Gallapagos Islands, under the equator, was from ten to twenty degrees below that of the Gulf of Mexico, and the waters of the Gulf Stream on the coast of the United States. Moreover, the observations had shown a great difference in the temperature as taken on opposite sides of these islands. This difference he had ascribed to the low temperature of the inflowing current from the southern hemisphere, which from the observations obtained he had supposed to sweep near these islands. Analogous effects had also been noticed on the western coast of the African continent; and the obvious bearing of the observations which he had collected from other parts of the Pacific was to establish the existence and approximate outlines of a general system of currents in that vast sea, and which, as in other oceans, controls and modifies the temperature of its waters, most remarkably, even on the same parallels of latitude.

He had mapped out the results of his former inquiries on this prevailing system of both winds and currents on blank charts furnished by Mr. Dana, which were intended for the use, as well as correction, of the observers of the Expedition. These maps he regretted to say had been lost before the end of the cruise, in the wreck of the Peacock. He was now pleased to find, however, that the main results as to ocean temperature and currents, which had cost him no inconsiderable labor at earlier periods, no longer rested on scattered observations made by different navigators; but that Mr. Dana, among other highly valuable labors, had carefully examined and brought to bear on the case the undeniable evidence of innumerable *register thermometers* afforded in the living corals, which serve to mark with greater certainty and precision the extent and boundaries of the warmed waters of the Pacific Ocean.

Mr. John L. Hayes differed from Mr. Dana on the question of general elevation and subsidence in the areas of coral islands; his belief was, that according to the views of the great Prussian geologist Von Buch, the districts of elevation were limited and not contemporaneous.

Prof. Rogers enquired of Mr. Dana what was the maximum rate of subsidence consistent with the growth of corals.

Mr. Dana replied that there were no definite facts bearing on the subject, but that it must of necessity be very gradual.

Prof. Rogers remarked that if accurate measurements could be made for a sufficient period of time on existing reefs, of the depth of the water from fixed points, evidence might be accumu-

* See Mr. Redfield's paper, at page 293 of this volume.

lated equal in value to that derived from similar means on the coast of Sweden, as bearing on the great dynamics of our earth.

Prof. Bailey read a paper referring to the observations he had made upon the microscopic fossils in specimens from the infusorial stratum and adjacent miocene deposits at Petersburg, Va., which had been sent to him for examination by M. Tuomey, Esq. of that place, the discoverer of the locality.

He stated that the infusorial earth, and the included casts of Crassatella, Pecten, &c. contained all the species of Coscinodiscus, Actinocyclus, Dictyoca, &c. which he had previously described as characteristic of the deposits at Richmond and Rappahannock cliffs; and that in addition he had detected many novel and interesting forms, figures of some of which were exhibited—among them a Triceratium, a large species of Zygoceros, analogous to forms found living at Boston, and a curious unknown fossil which he suspected might be a portion of a Zygoceros.

In the infusorial specimens he detected no traces of Polythalmia, but found them abundant among the contents of the shells sent by M. Tuomey from the miocene beds of Petersburg—figures of some of these were shown, among which were species of Textularia, Rotalia, Triloculina, and also of a minute crustacean, resembling a Cypris in form, but which Mr. Dana had informed him might be analogous to the marine genus Cytherina.

Prof. B. then stated that he had detected Polythalmia, in a specimen of shelly limestone in the collection of Dr. Chilton in New York, which was used in the construction of the Alamo, and which was probably quarried in the neighborhood of the fortress. The species appeared similar to those of the cretaceous group. He also stated that he had examined a specimen of lignite from Cape Sable, Md., sent by M. Tuomey, and found it decidedly coniferous. As amber occurs at the same locality, he suggested that these coniferous trees might have produced the fossil resin which it accompanies.

Prof. H. D. Rogers then read the following letter from his brother, *Prof. William B. Rogers* of Virginia, on the limits of the infusorial stratum in Virginia.

University of Virginia, April 23, 1843.

Since my first discovery of the infusorial stratum on the Rappahannock and at Richmond, as referred to in my Report for the year 1840, I have succeeded in finding a similar deposit at numerous other localities, extending from the Potomac River to near the southern boundary of the state. Among these points may be enumerated the Stratford cliffs on the Potomac, the vicinity of Westmoreland Court House, and

a great number of localities between the Potomac and Rappahannock Rivers, the James River below City Point, Petersburg on the Appomattox River, and a tract about Dupre's bridge on the Nuhenen River.

Further search will, I am convinced, greatly multiply these localities, and the observations already made are quite sufficient to prove the wide horizontal extension of this interesting division of our tertiary series. Although in some of these localities, as at Richmond, the stratum reposes upon beds containing eocene impressions, and although beneath the miocene strata, at other places, as for example the Stratford cliffs and Petersburg, it is underlaid by unequivocal miocene, and hence at these places, if not generally, is to be referred to a position in the geological series within and near the bottom of the miocene division of the tertiary, I am inclined however to the opinion that these strata are not all upon exactly the same horizon, and that some of them lie in a higher part of the formation. M. Tuomey of Petersburg, who has recently observed the deposit at that place estimates its thickness at thirty feet.*

In connection with these statements it may be interesting to add, that accompanying the infusorial material I have found vegetable remains at some localities in great abundance. They are imperfectly carbonized, still preserving their form and the fibrous texture, and they seem all to be referable to creeping and apparently cryptogamous plants. From the specimens I am now collecting, I hope to be able to decide with some certainty as to their true character.

Mr. Dana remarked that he had observed the same form of microscopic shell, as No. 7 of Prof. Bailey's figures, in Oregon.

Prof. Bailey said the same form had also been found on the coast of England, probably washed from the chalk.

Mr. W. C. Redfield read a paper entitled "remarks on some new fishes and other fossil memorials from the new red sandstone of New Jersey."

In this paper Mr. R. alluded to the general absence of fossils in this formation and the enhanced geological value of the few fishes and other remains which had been brought to light, and submitted to the Association specimens of three new species of these fishes which he had obtained from near Pompton in New Jersey. He referred to the allied characters of these fishes with specimens which he submitted from the new red sandstone formation of England, and particularly to the slightly heterocercal character of their caudal structure. He showed their dissimilarity to the more heterocercal forms of the fishes of the coal formation, even when of the same genus, and their want of analogy to the

* See this Journal, Vol. xliv, p. 339.

homocercal forms of the fishes which are found as we ascend above the lias. He referred likewise to the Ornithoidichnite which he had found at Pompton, and submitted the same to the inspection of the Association, it being the first of these footmarks which had been discovered in the red sandstone of the Middle States of the Union; and noticed in connection with these tracks, the interesting discoveries of the bones of a gigantic struthoid bird, which formerly existed in New Zealand, the *Dinornis* of Prof. Owen, a good account of which was to be found in the Penny Cyclopædia for March, 1843, Vol. xxvi, p. 518.*

In continuation Mr. R. then referred to the fossil rain-marks which are found in the same rocks, and submitted some remarkably well characterized specimens, from different parts of New Jersey and Massachusetts. He showed that an objection which had been made at the last meeting to the genuineness of these rain-marks, founded on their appearance in relief on the upper surface of the rock, in some observed instances, could not be sustained; for the fact proved to be, that in a great number of cases in which soft unburnt bricks were exposed to rain, the secondary effect of the rain was to wash and denude the first markings in such manner as to leave only protuberances or markings apparently in relief. He found that the circumstances most favorable to the preservation of distinct pitted impressions of rain-drops and true casts of these in relief, were of somewhat rare occurrence.

Prof. Rogers said that he believed it was the general opinion of our geologists that the new red sandstone of Connecticut and New Jersey belonged to the upper division of the formation of the same name in Europe. One fact which went far toward deciding this point was the occurrence of the Posodonomya keuperi in the new red sandstone of Virginia, discovered by his brother, Prof. Wm. B. Rogers.

The Secretary then read an interesting letter from Prof. Owen, on the Ornithichnites and Dinornis, [which is inserted in the present volume of this Journal, p. 185.]

Prof. John Johnston of Middletown confirmed the statements of Mr. Redfield touching the numerous impressions of shrinkage marks, &c. observed in the new red sandstone at that place.

Prof. Rogers remarked, that the cracks in the new red, were proofs of long continued dry weather, and their width might be proportionate to its continuance, while the size and depth of the rain-drops gave evidence of the strength of the shower ensuing.

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* See this Vol. p. 185.

Mr. John L. Hayes, from an extensive observation of the feet and tracks of living birds, was led to the belief that the Ornithichnites of Hitchcock were probably impressions of volant Grallæ, as these birds inhabited low and marshy ground, while the heavy Struthoid birds allied to Apteryx and Dinornis, were the frequenters of arenaceous plains and lofty hills.

Dr. Emmons showed specimens from the Potsdam sandstones, having strong impressions resembling rain-marks—proving the existence of these ancient meteorological registers, and of course of the rains producing them, much lower down in the rocks than heretofore observed.

Prof. Hitchcock then exhibited casts of nearly all the varieties of bird-tracks hitherto discovered in the Connecticut sandstone. These casts had been skillfully prepared and grouped by his friend Dr. James Deane of Greenfield, the original discoverer of the tracks. He said he could not but feel that the Apteryx character of the impressions, taken with the discovery of the Dinornis, had had a great influence on the mind of Mr. Owen, as deciding his final conclusion. He had himself been so much impressed with the mammalian massive character of the Ornithichnites giganteus when first discovered, that after an attentive consideration he rejected the specimens, in the belief that no bird could make so bold and deep an impression. He could not believe that the birds which made such impressions were volants; while on the other hand, some of the impressions were so delicate and slight as to equal the tracks of any of the volants of the present day.

Dr. Jackson read extracts from a letter of Elie de Beaumont, expressing his great interest in the specimens of O. giganteus which Dr. J. had sent him, and his belief that they were animal tracks.

In the course of this discussion Ex-Governor SEWARD had been introduced to the Association by Prof. Emmons. He was addressed by the President, who expressed in the name of the Association the great obligations American geology owed to him for the zeal and fidelity with which he had carried the New York survey to a successful completion.

To which Gov. Seward briefly replied.

Subsequently Gov. S. was elected a member of the Association.

Mr. John L. Hayes read a report, prepared in pursuance of a resolution of the Association, upon the probable influence of icebergs upon drift.

The information presented had been obtained from an examination of more than eighty persons, principally masters of vessels engaged in the whale and South Sea seal fisheries, in the merchant service and Labrador fisheries, all of whom had seen icebergs; also from authentic published accounts. He adverted to the intense interest with which all glacial agencies were regarded, to explain the various phenomena of drift, the transportation of earth and large fragments of rock in a southerly direction, the abrading and furrowing of the rocks in the same direction, the distortion and bending of strata of clay, the formation of bowl-shaped cavities, and of the peculiar longitudinal ridges of bowlders and gravel which occur in the drift.

To throw light upon these phenomena, he had collected information,

1. As to the mode of formation of icebergs, their original position, and the manner in which they had been detached.

2. The magnitude and form of those floating at sea.

3. The direction, rate, and nature of their movement, the limits of their transport, their grounding and dissolution.

4. The positive and negative testimony as to the transportation of fragments of rock and earth.

In the first place, it was shown by accounts given of northern and southern glaciers, that islands of ice are fragments which have been detached from those glaciers—that the fixed icebergs or glaciers of the Arctic and Antarctic shores are governed by the same laws, and exhibit the same general phenomena as the glaciers of the Alps. Like the Alpine glaciers, the fixed icebergs of the north and south polar shores are formed by the yearly accumulation of snow. Blocks of rock and earth are found on their surface and in their interior, as they are found on the Alpine glaciers. Several of the fixed icebergs of the Antarctic were particularly described. Instances were cited where these fixed icebergs or glaciers had been strewn with stones transported from a distance, which stones had been afterwards covered by new deposits of snow and ice, where large rocks were found in the perpendicular face of the glacier overhanging the sea, and where they have been covered with piles of sand and volcanic scoriæ. It was shown from the peculiar structure of these icebergs, their fissures, &c. that they must advance into the sea precisely as the glaciers of the Alps do along the valleys. Instances of the detachment of icebergs from the glaciers were cited, and the immense waves produced from their fall were described—these waves lifting up large vessels upon the shores, detaching other bergs and dashing them to pieces, and loosening from them imbedded fragments of rock.

2. The enormous mechanical power which might be exerted by moving icebergs, was inferred from their great magnitude. Many were

described from oral accounts over two hundred feet in height, and from two to fifteen miles in length; and some, which from careful admeasurement, were found to be from two to thirteen miles in length.

3. With regard to the nature of their movement, it was observed that it was very slow and perfectly steady, and in the direction of the great under currents which always tend from the poles to the equator, and that no rotary movement had ever been noticed; and indeed no remarkable movement except that produced by the overturn of the iceberg. Facts were mentioned illustrating the great depth of water in which icebergs sometimes ground, and that they thus remained grounded many years. The limits of the transport of icebergs were shown from the facts observed to be about 40° of north latitude and 36° south latitude.

4. Many original facts were stated as to the transportation of bowlders, from a small size to the diameter of many feet, and from an examination of the positive and negative testimony upon the subject, it was inferred that icebergs are rarely seen charged with foreign materials except near their source.

From the facts exhibited, the following inferences were drawn:

1. The steadiness in the movement of the icebergs in our present seas, in the direction and under the influence of great under currents in our northern hemisphere from causes which must have prevailed as well in the ancient as in our present seas, favor the theory that icebergs with gravers of rock in their lower portions, or pressing the sand and gravel by their immense weight along the surfaces of the rocks in the bottom of the ancient oceans, might have scored and grated along the rocks, grinding off their salient points, and leaving their surfaces smoothed and striated in the fixed southerly direction in which they now occur.

2. The immense magnitude of the icebergs in our present seas, and the evidence as to their present mechanical power, when moved by powerful currents, warrant the conclusion that they must have exerted a powerful influence in pushing and crowding along the sand and gravel which formed the bottoms of the ancient seas, and in thus forming accumulations analogous to the moraines of the glaciers.

3. The length of time during which icebergs may remain aground, even when swept by rapid currents which might surround them with sand and mud, or sweep away the loose materials, leaving hills or banks upon spots protected by the stranded icebergs, favors the idea that this agency had an influence in giving the present form to our drift.

4. The formation of glaciers upon the present coasts under such circumstances, that fragments of rock and detritus from the land upon which they form becomes attached to them, the constant advance and

separation of the glaciers from the land, and then floating into the sea as icebergs, with loads of earth and rocks, lead to the conclusion that icebergs, breaking off from the shores of ancient seas, were important agents in the transportation of rocks and earth from their parent beds. The existence of immense fragments of rock in situations where they could not have been carried by water alone, as on the sides of hills with valleys intervening between them and their parent beds, but where they might have been left by stranded icebergs, favors this conclusion.

5. The fact that a large part of the fragments detached from glaciers are of small size, and that these small fragments of icebergs or glaciers are dissolved and broken to pieces at no great distance from the parent glaciers, together with the fact that fragments of rock, although often seen near their source, are rarely seen at a distance, lead to the inference that the same causes limited the transportation of the bowlders and larger fragments of the drift, to within the comparatively small distance from the parent rocks at which they now occur.

Mr. Nicollet, from the committee on drift, had drawn up a short paper on the erratic deposits of the great valley of the Mississippi, which however he did not read, but made some remarks on the great importance and interest of the subject, and its bearing on the philosophy of geology and present causes. He urged the importance of an united effort on the part of all of the members of the committee on this subject, and the advantages of their making up a general report on the subject to embrace the views and observations of all.

The standing committee then handed to the chair the following resolution, which was passed, and the letters mentioned addressed.

"The standing committee has come to the conclusion that from the number of papers to be presented, it is inexpedient to accept the polite invitation of the railroad companies. Therefore resolved, that the secretary address a letter of thanks, making known the regret of the Association, and that it be sent to the secretaries of the said companies at an early hour."

Dr. C. T. Jackson was on the committee appointed to report on the subject of drift; he had, from his labors in the field and laboratory, been prevented from giving that time to the preparation of a paper on the subject which it seemed to require. He would call attention to a few subjects orally. The uniform direction of these scratches seemed to indicate a general cause acting in a direction from the north. In Lapland and Finland their course was found to be from northwest to southeast.

Dr. J. gave way to a motion that the Association now take its recess.

Afternoon.—Mr. S. B. Buckley read a paper on the *Zygodon* of Owen, (the *Basilosaurus* of Harlan,) and exhibited some of the enormous fossil vertebræ of this extinct animal.*

Mr. Redfield spoke of the importance of retaining this skeleton in the country, and of its appropriateness, when suitably erected, as an ornament to this hall, which now contained so rich a collection of palæontological remains.

On motion of *Prof. Hitchcock*, a resolution was adopted expressing the sense of the Association as to the importance of having this skeleton placed in the state museum.

Prof. Bailey said that it was interesting to see the two extremes of existence united in the same specimen; he had examined the marl in which the bones of the Zygodon were imbedded, and found it composed to a great extent of minute Foraminifera and Polythalmia, some of which were perfect, others decomposed or showing only casts of thin cells. The species he did not recognize as having seen before.

Dr. C. T. Jackson resumed his communication on the subject of drift, begun in the morning session, being one of the committee appointed last year to report on that subject.

Few subjects excite more attention at present among geologists than the phenomena of drifted rocks and soils, and the recent attempts to combine observations and form some plausible theory has invested it with a still higher interest. It was formerly supposed that the phenomena of drift were the effects of a transient deluge, which many were inclined to identify with that described in Genesis. It is now however generally conceded among geologists, that this occurrence was prior to the creation of man. There are no remains of his works or presence to be found in the tertiary shale which immediately preceded this epoch; neither arrow-heads, firebrands, or any other work, nor the vestiges of his footsteps or the fossilized bones. Hence we may conclude that man was not formed until the world was finished and prepared for his abode. The geologist sees in the diluvium not proofs of Divine vengeance, but evidences of the highest wisdom and goodness of the Creator in thus preparing and commingling the soils of the earth. The earliest exact observations on this subject were made by De Saussure, Pallas,

* See description of this skeleton by Mr. Buckley in this Journal, Vol. XLIV, p. 409.

and De Luc, on the continent of Europe, and by Sir James Hall in Scotland. More recently Von Buch has collected a vast amount of interesting facts, and lastly Messrs. Charpentier, Agassiz, Sefstrom, De Beaumont, Durocher, and many other distinguished men in Europe and America, have added their labors, both in the collection of facts and the development of theoretical views. It was natural that causes should be sought for effects observed, and not surprising that when bold and plausible hypotheses were advanced, they should find warm advocates, as well as uncompromising opponents, and thus from a thorough sifting of the facts and principles by both parties, truth would be in the end attained.

The present state of the controversy as to the phenomena of drift, aptly illustrates these remarks. A cause now in action was discovered, which was deemed sufficient to have produced the various effects which are termed diluvial, or drift phenomena. Many eminent men incautiously embraced the new theory, which within two or three years from its promulgation, has been found utterly inadequate, and is now abandoned by many of its former supporters. This was the glacial theory of the celebrated Agassiz of Switzerland. He then called attention to a report by M. Durocher on the phenomena as exhibited in Scandinavia, and a comparison of the facts there noted with what he had observed in the northern states of this country. De Beaumont objects to the use of the word *diluvium*, and prefers the terms employed by Charpentier, *terraines erratiques*, *blocs erratiques*, and *phenomene erratique*. In England and this country the term *drift* has been substituted, but to cover all the cases must be combined with some other word. Thus we say, *drift*, *striæ*, *embankments*, *ridges* or *phenomena*. Durocher uses the word *diluvium*, but by it he does not intend to express a belief in any theoretical views. He refers the phenomena to a period anterior to the existence of man, as is done in this country. In 1839, Mr. D. left the French Arctic Exploring Expedition for the purpose of investigating the *drift* phenomena in the Faroe Islands; then visited Spitzbergen, the northern coast of Lapland, St. Petersburgh, Finland, the interior of Russia, Poland, the north of Germany and Denmark, continuing his travels until June, 1840. Thus passing over an immense surface, he collected all the most important facts, and consulted the observations of the geologists residing in the various countries he visited. He found in the north of Europe, that the furrows and scratches are visible on all the rocks which are hard enough to receive and permanent enough to retain them. And also that there are two sets of striæ crossing each other at angles, never greater than 10° or 12°. This coincided exactly with the observations of Dr. J. on the drift scratches of Maine, New Hampshire, and Massachusetts. On the borders of the

Gulf of Alten, (the variation being 11° west,) the striæ on the sleat run north and south magnetically; those on the amphibole N. 15° W. and S. 15° E. by compass. On the borders of Lake Ladoga, the striæ on granite run N. 22° to 25° W. by compass. In Finland, the striæ are further from the meridian, being N. 69° W. to S. 69° E. by compass. Proceeding south, this angle diminishes to N. 20° W. and S. 20° E.—N. 30° W. and S. 30° E.; the normal direction being N. 25° W. mag.

M. Sefstrom found on the west side of the Gulf of Bothnia that the direction was N. W. and S. E. and this may be considered as the mean of all the observations; the variations being accidental, owing to elevations producing deviations in the course of the currents. Valleys of erosion follow the same direction. The direction of the *Osars*, and also drift-blocks and bowlders, correspond with this course. These *Osars* are identical with the ridges which in Maine are called *horsebacks*, and like them resemble railroad embankments, and are composed of sand and gravel.

Marine shells are found in the drift of Denmark, of which M. Beck recognizes seventy species as identical with those now living. They are often broken, but sometimes both valves occur together. Near Stockholm the shells were found whole, and were tranquilly deposited. So also at Uddevalea, originally explored by Alex. Brongniart. These are among the proofs of the submergence of this region during the diluvial epoch. From the S. E. of Finland to St. Petersburgh and Moscow, erratic blocks of granular granite, peculiar to Viborg in Finland, are found scattered—the least distance is from one hundred and forty to one hundred and fifty leagues. Blocks of sandstone at Memel came from Lake Onega, a distance of two hundred and forty five leagues. Thus the drift phenomena in the north of Europe are more remarkable than those described by Dr. J. in the Reports on the Geology of Maine, in which erratic blocks were traced to a distance of one hundred and twenty six miles in a direction S. E. by E. Examples of these were mentioned in Maine, New Hampshire, Massachusetts, and Rhode Island; and reference was made to the works of Prof. Hitchcock for other striking instances.

Messrs. De Beaumont and Durocher are equally cautious in adopting any one cause for the phenomena. Durocher thinks there is abundant proof of two separate causes, distinct as to the time of their occurrence. 1st. The breaking up of the Northern or Frozen Ocean, by which a current loaded with ice was sent over the partially submerged country, abrading the rocks, producing striæ and bowlders, and carrying them to the S. E. 2d. That afterwards icebergs formed on the coast and carried off each summer their load of rocks and earth, and deposited them to the S. E. In the meanwhile the land was gradually rising from

the sea level, and brought the detritus above the surface. During the periods of tranquillity marine shells were deposited in those portions of the country which were submerged. Durocher, De Beaumont, and Bohtlink, all consider the glacier theory insufficient to produce the effects observed. And the phenomena in Scandinavia, like those observed here, all go to disprove the hypothesis of Agassiz. Even in Switzerland, the celebrated Prof. Andre De Luc regards the views of Agassiz as fanciful and imaginary. Dr. J. referred to the quotation of Peter Dobson's hypothesis by Mr. Murchison. He concluded by remarking that he had travelled over most of the Swiss glaciers, and knew the statements made were greatly exaggerated. He knew too that our drift striæ and erratic blocks did not radiate from one principal mountain group, nor were they in any case much deflected by them; nor was there any proof that glaciers had ever existed in Maine, New Hampshire, or Massachusetts.

Mr. Nicollet then rose and addressed the meeting at considerable length and with great animation on the subject of Dr. Jackson's paper just read, and in opposition to the glacial theory of M. Agassiz. He expressed his astonishment that M. Agassiz should have entirely overlooked the labors of his predecessors in the same field, and particularly of M. De Saussure, who spent forty years in investigating all their phenomena, and had nearly exhausted the subject. It was impossible to conceive how the effects ascribed by M. Agassiz to the moving glaciers could with propriety belong to them. The *mer de glace* was an immense vault of ice under which, as in a grotto, one could walk even for twenty miles, while on its bottom runs a stream of water. How could the bottom of the *mer de glace* then be supposed to score and furrow the rocks in its path? M. Agassiz had overlooked too the true effect of the expansion of the ice; he had ascribed to *it* the downward movement of the glacier, while De Saussure long ago proved that this motion was due to gravity only. The expansion did effect the fissuring and arching up of the glacier, just as it produces the same phenomena in the ice of our rivers.

One very important point in the subject of diluvial furrows had been overlooked by M. Agassiz as well as by many other observers of the same facts, not only in the Alps but in other places—this was whether the furrows on the rocks obeyed the direction of the valleys, or the general direction of diluvial furrows, irrespective of the sides of mountains and the course of

valleys. Until full observation was made of the facts with this point in view, we could arrive at no valuable general conclusions.

An animated and pleasing debate then ensued between *Prof. Hitchcock*, *Mr. Nicollet*, *Mr. Redfield*, and the Chair.

Prof. Hitchcock remarked that so disastrous had been his experience in respect to the glacial theory of Agassiz, that he was almost afraid to say any thing more on the subject. His views had been so much misunderstood on both sides of the Atlantic, that he was satisfied that the fault lay in the language which he had used on former occasions. He had been supposed to be an advocate for the unmodified glacial theory. But if he could trust his own consciousness he never had been a believer in it. The views which he presented in his paper on the phenomena of drift in North America, read to this Association last year and now published in their Transactions, are essentially the same as those which he held when he gave his anniversary address before this body in Philadelphia: and those views he certainly stated in that address. Nay, he invented a new term, viz. *glacio-aqueous*, to express the final conclusions of his mind on the subject. By this term he meant to say that the phenomena of drift were the result of the joint action of ice and water, without saying which of these agents had exerted the greatest influence. But whether that glacio-aqueous action had been the result of the enormous accumulation of glaciers according to Agassiz, or from floating icebergs while northern countries were yet beneath the ocean according to Lyell and Murchison, or of the upheaving of the Arctic Ocean whereby its aqueo-glacial contents were precipitated southward according to De la Beche, he had not then made up his mind nor has he yet made it up. The *Etudes sur les Glaciers* of Agassiz did indeed throw a flood of light into his mind, by showing how (if that writer has rightly interpreted the phenomena of glaciers) moving ice could produce such effects as are connected with drift. It did seem to him to have introduced a new element into the dynamics of drift, and he expressed his strong admiration of the labors of the distinguished professor of Neuchatel, though he certainly never meant to adopt his views in full: and in saying that the fundamental principle of Agassiz's theory seemed to him to be true, he meant only that ice and water had been the agents employed in producing the phenomena of drift, for he understood the glacial theory to require both these agencies. Indeed the melting away of the vast accumulations of ice around the poles, which this theory supposes to have been done suddenly, must have produced southerly currents and transported icebergs in that direction in vast quantities.

The ground then which he (Prof. H.) took and still takes was that ice and water were the agents employed in producing the phenomena of drift, and he found that nearly all geologists of the present day admit this position. He was happy therefore to find his views in accordance with the whole geological world. Geologists do indeed differ in the proportions in which they mix ice and water for this work; but they all admit both agents to have been concerned. Even Elie de Beaumont, according to Dr. Jackson's paper just read, admits both these agents, and this certainly is an advance upon the views which have so extensively prevailed in continental Europe on this subject. Prof. H. did not feel as if we could safely go farther than to say that drift was the result of glacio-aqueous action, and he had some doubts whether we could ever go farther except hypothetically. Yet most geologists seemed not willing to stop there: and he had no objection to their indulging in conjectures in the wide field beyond, and he would always be happy to examine their ingenious hypotheses. And in regard to the glacial theory he could not agree with Dr. Jackson, that it was already dead and waiting to be buried. The late numbers of the Edinburgh Philosophical Journal, of the Geologist, and other European periodicals, loaded as some of them were with papers on this subject, certainly looked as if some vitality still remained in that theory or its advocates. He was particularly interested in the effort of Mr. Maclaren to make the glacial theory and the iceberg theory coalesce. Indeed it would not be strange if the true and ultimate theory on this subject, if that is ever reached, should be a combination of all the three leading hypotheses which have been alluded to above.

In conclusion, he begged leave to say, that he derived his first ideas of glacio-aqueous action, nearly twenty years ago, from the papers of Sir James Hall on the diluvial phenomena of Scotland. For although that writer imputes those phenomena to a deluge, he loads the waters with ice bearing fragments of rock which must have scoured and ground the surface. And it is a little curious that while the late distinguished president of the London Geological Society finds the germ of the iceberg theory in a paper by one of our countrymen, (Mr. Dobson,) some of us should have derived it from a distinguished geologist of Great Britain.

Mr. Redfield said if any savans of Europe have set down our worthy first president (Prof. Hitchcock) as an incautious geologist or likely to be led away by novelties, we all know they are much mistaken. He well recollected having been led into a discussion with him on the subject of the transportation of bowlders and drift some years since, on which occasion the agency of ice had been treated with proper scrutiny and caution. Mr. R. said that although then entertaining views similar

to those of Lyell and Murchison, yet he had relied mainly on the force of the polar currents. But as to the *cause* of these currents, which had been previously referred to, he desired to wash his hands of all suspicion of attributing them to the melting of ice and evaporation in the lower latitudes. He considered them to be due to other causes of a far different nature.

Mr. R. spoke of the vast effects of the regular polar currents of the ocean in the transportation and deposit of bowlders and drift, currents which must have been in action ever since the earth had rolled on its axis with an incumbent ocean; and he thought this mighty and enduring agency as conjoined with ice had not been duly appreciated. He felt that much credit was due to Mr. Hayes for the facts which he had collected from observant voyagers, and took occasion to allude to the summary outlines of the systematic currents of the ocean which had been given by himself at a former meeting of the Association, but not furnished for the report of proceedings. He then traced on the map the natural course, as well as the deflected or forced direction of these currents as they issued from the Arctic regions; the natural course falling westerly and the deflected one easterly of a meridian line, and corresponding severally in direction with each of the two systems of striæ found on the rocks of North America. He showed on the map the coincidence of these striæ with the present courses of the great ice-fields and numerous icebergs of the north; suggesting that attention to the phenomena of single icebergs in open sea would fail to produce an adequate conviction of the efficiency of the cause in question. But those who had attentively considered the narrative of the last voyage of Capt. Back might be satisfied that the movements in mass of such vast *packs* of ice and icebergs as those in which his ship was enclosed for many months and moved slowly a great distance to the southward by the force of the great current and the agitation produced by storms, doubtless while rending and moving by means of the vast floes and the base of the bergs the incoherent portions of the shores and the subaqueous topography and grooving the faces of the coherent rocks, were causes which acting without stint of time, were sufficient to produce most of the phenomena which have been noticed in drift formations.

Mr. Hayes said his observations had proven to his satisfaction that the immense mass of these icebergs below the water caused them to be entirely influenced by the currents beneath the surface, and explained why apparently they were not affected by the winds and currents above the surface. He alluded to some facts which had been observed as illustrative of this point. The reason of these strong under-currents he did not undertake to ex-

plain. He would ask of Mr. Redfield whether in his opinion there was any reason to suppose that the currents in the ocean below the surface, always tended towards the equator.

Mr. Redfield replied at some length, explaining the geographical system of currents observed in the ocean. These were mainly independent of the atmosphere and winds. The whole of the evaporation at the equator, let it be as much as it may, would have no sensible effect in producing a current from the polar regions to supply its place. The Mississippi, through its numerous branches, received the drainage of many thousands of square miles, and some had contended that the influence of its current must materially affect the force of the Gulf Stream. He would not undertake to say but what *some* effect was produced, but he could say, that it had never yet been perceptible to observation.

In reply to a question of Mr. Horsford, *Prof. Espy* stated that the evaporation at the equator could not be the cause of the polar currents towards that point. A current of one mile per hour would fill up the entire deficiency caused by the evaporation, in one hour. It was idle therefore to look to that cause for the effect. He thought the mean temperature at the poles and equator, a sufficient cause to explain the phenomena of the currents. Mr. E. explained this point at some length. The specific gravity of the water, caused by the difference of temperature, would give a current from the poles to the equator. At the equator, the temperature of the water at the surface was about 70°, while at the depth of one thousand fathoms it was but 37° or 38°, and remained at about that temperature as you went to a greater depth. This could only be accounted for by supposing that the water at that depth was supplied by a current from the polar regions.

Mr. Hall read a short paper and presented a natural section of a portion of the shore of Lake Erie, exposing the broken and contorted strata and intermingled drift.

This section was exhibited to the Association last year, with a view to elicit similar facts and to enable us to draw some inference as to the cause producing the phenomena.

He stated that the subject of drift had occupied but a small portion of his attention, having been engaged mainly in the study of the older rocks and their contents. In the outcropping edges of the limestones and other firm strata, he had frequently noticed the separation of the layers, one being elevated at a much higher angle than the next below,

and the space filled with loose materials. Sometimes large masses were thrown over upon the stratum in place to the south—at other times they had been lifted and fallen back, presenting abrupt anticlinal axes of small extent.

In the section before us similar causes seem to have operated in a more stupendous manner, and to have produced corresponding results.

He gave an explanation of the section, which consists of a stratum of loam, resting on clay and gravel, and below this the shaly strata, which are cracked throughout into short blocks as if by a violent undulatory motion; insinuated beneath these, and between different strata, we find clay, gravel and pebbles, with smaller fragments of the shale. These materials are often folded and cortorted in such a manner as to lead to the conclusion that they could only have been produced by a tremendous force from the northward, forcing the loose materials beneath and between the strata, and moving the whole to the southward, producing the folded appearance.

The surface of the firm rock beneath is grooved and striated, precisely in the same manner as the surfaces of our present strata, so that if the broken rocks above were removed the surface would present the same appearances as those of the grooved and polished rocks.

It is here quite evident that the breaking up of the strata, the intermingling of the drift, &c., all took place during a single period, being produced by a force moving to the southward. This force, whatever it may have been, seems sufficient to have produced the breaking up of strata, the production of worn fragments, and the excavation or denuding operations every where visible.

The phenomena presented in this instance lead us to the most interesting conclusions, but whether we are warranted in assuming that similar causes may have produced the effects every where, it is perhaps impossible to decide. The subject of drift and the causes of its production are still open to discussion, and no theory yet advanced seems satisfactory to all parties. Therefore, without a desire to advance any theoretical considerations, he had presented the facts for the consideration of the Association.

After explanation of sections of the drift and river channel at Portage, *Mr. Horsford* inquired whether there was evidence of the original north and south valleys having been excavated by a force operating from south to north.

Mr. Hall replied that he had formerly embraced that opinion, but finding no positive evidence of such a force had abandoned it. If this opinion were true we should expect to have found remains of southern drift among the loose materials at the north, but so far from this being the case he had always found northern mate-

rials in the southern drift. He was led to believe from the facts in the case, that there must be valleys of two distinct epochs.

Prof. H. D. Rogers then addressed the meeting in a very eloquent manner, showing how the absence of southern materials in the northern drift might be accounted for by supposing the forces of the northern currents to have been so great as entirely to have swept all vestiges of the superficial drift into the Gulf of St. Lawrence. This vast reflux of waters was attendant on the uplift of the continent, and the great drainage which resulted from the flowing off of the oceanic waters.

Mr. Hall read a short paper in explanation of two sections at Portage, N. Y.

Under the terms diluvium, drift, &c., are included products, which, however similar they may be in general characters, are often due to different causes, and are the results of eras widely separated in time, and differing in many essential circumstances. The more ancient appear to have been the more universal, and as we descend to modern periods, the extent of the operation seems to have diminished.

In our theories we have made provision for a wide sweeping deluge, for immense excavating waves, and for hemispheres of ice, but we have overlooked the subsequent and minor, though often important operations of the bursting of lakes, or the change in river channels, which must have occurred frequently during the earlier periods after the emergence of our continent from the ocean. The existence of such lakes, would be a natural consequence of the contour of the surface.

Evidence of the outbreaking of such lakes is seen in the margins of all our great valleys, where more recent detritus is spread over the older deposits of that kind.

Bones, shells and fragments of wood, are frequently found in these deposits, which are referred to the drift period—though we are not prepared to say that the drift is destitute of such remains, yet those which Mr. Hall had seen were clearly in positions to be referred to a subsequent period.

In the excavation of the Genesee Valley canal, at Portage, along the side of a hill which consists of alternating layers of fine sand and clay, at a point about two hundred feet above the base of this deposit, some fragments of fine-grained wood, highly impregnated with iron pyrites, were found. This was in a layer covered by a mass of gravel and sand eighty feet thick, which, from the nature of its materials, was a deposit subsequent to the drift, and of southern origin.*

* The existence of these two deposits was pointed out in the Annual Geological Report for 1839.

Upon the surface of this sloping hill is about ten feet of a deposit of sand, clay and gravel, saturated with water, and constantly moving down the side of the hill. This is evident not only from facts ascertained in excavating the canal, but also in the fact that the oak trees which grew upon the high ground have slidden down and become intermingled, standing in all directions among the hemlocks which skirt the margin of the river below.

The deposit in which the fossil wood is found, was probably made in a lake which was afterwards filled with an accumulation of gravel and sand, derived from the outbursting of some reservoir farther south. This deposit being pervious to water allows this liquid to pass through it, and meeting with an impervious mass below, flows out in the form of springs, undermining and carrying down the surface matter.

In a transverse section extending across the river valley and channel, Mr. H. illustrated the changes which had taken place in the direction of the Genesee, during a comparatively recent period.

The river to the south of Portage flows in the bottom of a broad valley, extending toward the north. At Portageville the stream bends around to the left, and after flowing a short distance nearly south, turns to the north and northeast, cutting its channel through the rocky slate in some places to the depth of three hundred and fifty feet, and forming in its passage three falls of sixty six, one hundred and ten, and ninety six feet respectively. This channel is narrow with mural banks; but a short distance below the lower fall it emerges into a broader valley, in a line with the channel to the south of Portage, before it is deflected from its course.

The space between these two points, as shown in the section, is a deep broad gorge filled to a great height with clay, sand and gravel.

This is evidently the ancient channel of the river, and yet after it had become filled with this drift the stream found an easier passage by excavating the solid rock for three miles, than by removing these loose materials.

Still below this point the river leaves the broad channel and excavates a gorge through the shales emerging into the broad valley at Mount Morris.

Several other lakes and streams in Western New York exhibit the same phenomena, and although there are northern channels filled with drift, the streams often turn at right angles and excavate their course through rocky strata.

The Chair mentioned to the meeting that Prof. Hitchcock would favor the Association with a public lecture in the evening, at 7½ o'clock. Adjourned.

Monday, May 1, 1843.—The minutes of Saturday were accepted, and the Chair presented from the committee on publication the completed volume of transactions, comprising the proceedings of the Association from its organization to the present time, together with all the papers which have been read at former meetings of the Association. It was ordered that the present committee on publication be continued for the year to come, and that Dr. Amos Binney be the treasurer of that committee. It was also

Resolved, That the thanks of the Association be presented to the committee of publication for the very acceptable manner in which they have performed the laborious and responsible duty of publishing the first volume of the Transactions of the Association.

Resolved, That the thanks of the Association be presented to Prof. E. Hitchcock, for his very interesting address before the Association on Saturday evening, and a copy be requested for publication.

Prof. Hitchcock read a paper on native copper found in drift in Massachusetts, and also on the occurrence of yttro-cerite in the same state. He considered the copper as having originated in the primary to the north of the place where it was found; there was no drift in that direction, having any other than a northern origin; he considered this fact might have an important commercial bearing.

Dr. Jackson stated he had found the yttro-cerite in Bolton, Ct.

B. Silliman, Jr. reminded Prof. Hitchcock that copper was found at Bristol, Conn., in the primary, beyond but near the junction of the new red, and that Prof. Shepard had expressed the opinion in his report, that all deposits of copper in the secondary were limited in extent, and were originally derived from the primary.

Dr. L. C. Beck stated that native copper had been found near Somerville, N. J., under circumstances similar to those in Massachusetts and Connecticut; one mass in his hands weighed when found, one hundred and twelve pounds.

Prof. H. D. Rogers enquired of Dr. D. Houghton if he considered the native copper of Lake Superior, as belonging to the older secondary, and whether the trap and sandstone of the peninsula of Michigan, were of the same geological age as the similar formations of New Jersey and Connecticut.

Dr. Houghton said he could not speak definitely as to the contemporaneousness of the two formations, but he was sure of the *similarity* of their structure. The copper in Michigan was frequently accompanied by zeolites and prehnite.

B. Silliman, Jr. reminded Dr. H. of the discussion on this subject, at the meeting in Philadelphia in 1841, and stated his analysis of the Lake Superior copper in reference to its being an alloy of silver and copper, and also the silver on the copper to ascertain if it were an alloy of copper and silver; but he had found both the metals distinct and quite pure, although fused in to perfect union at their two surfaces. He would enquire of Dr. H. whether he had found silver under similar circumstances, and whether the silver was not segregated by the action of the dyke, and found in separate masses, and if it promised to be of any economical value.

Dr. Houghton said the great mass in Yale College cabinet, referred to by Mr. S., was the only loose mass, where he had seen the silver and copper united; he had been at the same vein that afforded this specimen, and had found silver distinct in branching masses, but in very small quantity, and he had in one case found antimonial sulphuret of silver; the silver and copper were both very nearly pure.

Dr. Beck said the facts in N. Jersey were of a similar character.

On motion of the Chair, Dr. L. C. Beck, B. Silliman, Jr., and Dr. D. Houghton, were appointed a committee to draw up a report for the next annual meeting, embracing all the known facts, bearing on the occurrence of native copper, in the trappean regions of the United States.

Mr. James Hall showed a specimen of the cherty limestone from near Niagara, as proving in a very conclusive way, the direction of the diluvial agency by which the surface of the rocks *in situ* had been ground down; in this specimen the nodules of chert had resisted the diluvial action and stood out *in relievo* above the surface of the stone, having before them a ridge of limestone which had been protected by the chert nodule.

The Chair said he adverted to this fact last year, and considered it as conclusive evidence against the hypothesis of simple aqueo-glacial action, as the cause of the smoothed and polished rocks; he considered that such an effect could result only from the sweeping over the rocks of a vast accumulation of angular

and *loose* fragments of hard rocks, mixed with smaller gravel and sharp sand.

B. Silliman, Jr. stated that much of the loose material which covered the red sandstone of Connecticut, consisted of fragments of rocks, many of which could be referred to the trap mountains to the north, which presented one surface worn down quite flat, like the rubbing stone of a stone-cutter, and as if they had been carried evenly and for a long time, over the surface of the rocks, by some force competent to keep them in one position. He left it for gentlemen to decide as best suited their own views, whether they were thus held by being set in ice, or by a superincumbent and adjacent mass of loose materials and waters. The large pebbles of quartz and other hard rocks, of which the coarser beds of conglomerate in the Connecticut valley were composed, were worn down without dislocation, and had, measurably, served to protect a *lee* of adjacent rock from degradation in a manner similar to the cherty nodules shown by Mr. Hall. The striations in these valleys were about S. 20° E. and were on the whole irrespective of the directions of the valley, frequently scouring the sides of hills, in a line oblique to the axis of the valley.

The Chair urged that the diluvial currents had extended farther to the south in the long parallel valleys of Pennsylvania, and had been much influenced by the existing topography of the country; and the scourings on the rocks there were resultant lines between the general direction of the onward current, and the direction of the mountain slopes. It was uninfluenced by the topography of the country so long as the waters stood above the summits of the mountain ridges, but when the inundation was nearly exhausted, the subdivided current conformed itself almost entirely to the configuration of the surface.

Mr. Redfield said that the diluvial markings on our American rocks, might be viewed as constituting two distinct systems, in one of which the striæ have a southwesterly direction, in the other a southeasterly one; the latter system greatly predominating in the country lying east of the Hudson.

The discussion was continued for some length by Messrs. Houghton, Jackson, Espy, Rogers, Hitchcock and Hall, and it was generally admitted that we must find, in the conjoint action of water, ice and loose detritus, a cause sufficient to account for all the phenomena known by the various names of glacial, diluvial and aqueo-glacial.

B. Silliman, Jr. then made a few remarks on the configuration of the valleys of the secondary, as influenced by and connected with the intrusion of trap rocks.

The subject of appointing a committee on drift, to report at our next meeting, having come up for discussion, it was

Resolved, That the following gentlemen be instructed to report on the present state of our knowledge on the subject of drift. New England and New York, Prof. E. Emmons; the West and far West, J. N. Nicollet; the South, W. B. Rogers.

The chairman explained to the meeting, a view which had occurred to him to account for the crescent-formed dykes of trap in the new red sandstone of New Jersey and Connecticut.

That the crescent form of the trappean dykes of the New Red sandstone regions of New Jersey and Connecticut is in some manner connected with the dip of the stratified rocks which they traverse, is plainly indicated by the constant dependence between the direction of these crescents and the direction of the dip. Thus in New Jersey, where the dip of the Red Sandstone is towards the northwest, the horns of the crescents point towards the same quarter, while in Connecticut, where the strata possess an easterly dip, the points of the crescents are directed eastward.

May we not explain this curious relationship by conceiving the fissure through which the melted trap has pushed to the surface, to conform itself, where it traverses the upper part of the inclined sandstone, to the plane of the dip. The sandstone being disrupted in a plane parallel to the dip, the beds on the upper side of the sloping dyke will be lifted off from those upon which they reposed, and in this tilting of the beds, there will arise towards the extremities of the fissure seams or transverse cracks, extending in the direction of the dip. Now when we view the outline of the principal or central portion of the fissure continued into these transverse cracks at its extremities, we readily perceive that it must constitute a curve or crescent concave in the direction of the dip.

Mr. B. Silliman, Jr. remarked that there was an almost perfect identity between the views just explained by Prof. Rogers and those arrived at more than two years since by his friend Dr. James D. Whelpley and himself in the Connecticut valley. These views had been laid before Mr. Lyell by Dr. Whelpley, and illustrated to Mr. L. by visits to several localities in the vicinity of New Haven. It had been their intention to lay a paper on the subject before the present meeting, but they would postpone it to next year. It was then

Resolved, That Dr. J. D. Whelpley, Prof. H. D. Rogers and B. Silliman, Jr., be a committee to report on the intrusive trap of New Jersey and Connecticut.

Mr. Dana exhibited a few drawings by himself, illustrating the metamorphosis of the Anatifa. The young of the common barnacle was first noticed and figured by Thomson, who remarked its close resemblance to the species of the Cypris family among Crustacea. These drawings show the same with regard to the Anatifa, and prove also that the pedicle in the Anatifa corresponds with the anterior legs, (properly a pair of antennæ,) of the young Cypris-shaped animal. In the young state it swims free in the ocean and has a pair of compound eyes. The eyes disappear when changing to the adult form; in this state the Anatifa is a fixed animal, like the barnacle, and has no further use for eyes. One of the drawings represents the young free animal; a *second*, the same attached by its anterior legs, which terminate in a disk to the sea-weed, and a *third*, the full developed Anatifa, with the valves of the shell of the young, (the exuviæ,) loosely adhering to the foot of the pedicle.

The propriety of uniting these animals with the order Crustacea, has been often suggested. The structure of the mouth and legs, and the fact that they change their skin from time to time, like the species of this order, would alone seem sufficient to authorize this union; but now after the discovery by Thomson and others respecting the young, there is no reason for farther hesitation.

Dr. L. C. Beck read a paper on the occurrence of bituminous or organic matter in several of the New York limestones and sandstones.

In this paper the author stated that in almost all the New York water limestones which he had analyzed, the residuum left, after the action of dilute muriatic acid, when subjected to heat, gave out a bituminous or peaty odor. In some cases the proportion of this matter could be determined by first carefully drying the whole insoluble residuum, ascertaining its weight, and then subjecting it to a red heat and noting the loss. In other instances, however, while the bituminous odor was sufficiently evident, the loss of weight was scarcely appreciable. The same fact was observed in the limestone from Rochester, and generally in those limestones termed *fetid*. The sandstones at Laona and elsewhere in Chatauque County are often so highly impregnated with this or a like substance that specimens, even though kept for some time,

burn with flame. This bituminous matter has been observed in a limestone from Saratoga County, which, from the circumstance of its being made up of rounded grains, has been called *oolitic* limestone by mineralogists.

Dr. B. remarked that the odor of the fetid limestone had sometimes been referred to the presence of iron pyrites, giving rise to sulphuretted hydrogen. If it was due to iron pyrites, the sulphuretted hydrogen would have been evolved during the process of solution; which was not the case.

The author adverted to the statements made by Dr. Daubeny at the last meeting of the British Association (1842) concerning the occurrence of organic matters in various secondary limestones, and was inclined to consider the whole class of facts as proofs that races of organic beings had existed at the period of the formation of these rocks, of which not a single representative now remains.

Dr. Emmons remarked that this bituminous matter was of general occurrence in the New York rocks, even the older arenaceous deposits.

In some limestones noticed by Mr. Hall, the bitumen was so abundant as to run out when lime was burnt and render the specimen offensive to handle. The limestone at Montreal was so charged with bituminous matter as to smut the hands, while a case had been cited by the late Mr. Eaton, of a limestone having when heated the smell of horn, indicating the presence of nitrogenized matter.

Dr. Owen read a paper on fossil Palm Trees, found in Posey County, Indiana.

They were discovered about twelve miles from New Harmony, in excavating in a slaty clay on the banks of Big Creek, a tributary of the Wabash, for the purpose of laying the foundation of a saw and grist mill, and forming a rag dam. The stratum in which they are imbedded is one of the upper members of the Illinois coal-field.

From the first commencement of the excavation from twenty to twenty five fossil stumps have been seen. Dr. Owen has disinterred only three himself. These were found standing erect, with from five to seven *main* roots attached, and ramifying in the surrounding material. There is every reason to believe that if pains had been taken to expose the others, all would have been found provided with roots.

Besides the three trees which were transported to Dr. O.'s laboratory, several segments of other trees, previously dug out, were found amongst the rubbish. Some of these had the scars of the stems well preserved, and presented besides the structure of the bark, which re-

sembled minute wrinkles, something like the impression left on soft clay by pressing a file on it. No medullary rays or growths could be discovered on the transverse sections of the trees.

All the specimens observed had part of the bark converted into a black carbonaceous substance. Judging from the disposition of the scars on several of the specimens found, there must have been at least three species. This was shown on a drawing; and Dr. O. exhibited a model, quarter the natural size of the smallest stump excavated. A portion of one of the roots of the largest one was also laid before the Association. The general character of these interesting specimens was determined by the form of the scars, being longer horizontally than vertically, and the absence of flutings. In the bed of Big Creek, fifteen feet beneath the roots of the trees, is a seam of coal supposed to be from three and a half feet to four feet thick. Almost immediately over them is a layer of sandstone, and over that an imperfect seam of coal. The top of the fossil tree is about fifteen feet beneath the level of the bank of Big Creek.

Dr. Owen supposed from the present position of these trees, that they have been quietly submerged and now occupy the spot where they originally grew.

A more detailed description of this locality of fossil palm trees will probably appear hereafter in this Journal.

Dr. C. T. Jackson read a report on the organic matters of soils, and exhibited specimens of them and their characteristic salts and compounds.

He demonstrated the complex nature of mould or humus, and proved that only refined and exact analyses would show the causes of fertility and barrenness in soils. Several cases in point were cited, and among others the analysis of three soils; one of which was almost barren; the second was moderately fertile; while the third was remarkably productive and had been cultivated for nearly one hundred years. In these three soils the relative proportions of organic and mineral matters were precisely the same, insomuch that it was supposed at first that they were all taken from the same spot; but a more refined analysis showed a very marked difference in the condition of the organic matters, and to this difference, imperceptible to an ordinary or crude analysis, the difference in fertility was owing.

The organic matters formerly confounded under the names of ulmin, geine and humus, are numerous and very different in their chemical nature, varying in different soils and producing various degrees of fertility.

Berzelius who first described and named geine, has utterly abandoned that name* as improper, there being no such proximate principle, it having been proved to be a very complex compound of new and peculiar acids. Those which we know at present are the following: crenic acid, apocrenic acid, humic acid, humin, extract of humus, glairin, glucic acid, apoglucic acid and coal of humus.

The five first of these substances are generally present in fertile soils. Crenic acid forms two salts with lime, one of which, the crenate, is very soluble in water and in alcohol, while the subcrenate is more difficultly soluble.

Apocrenic acid is highly charged with nitrogen, and is a very valuable ingredient in soils, furnishing a portion of the nitrogenous matter of plants. It is probably formed by the gradual change of vegetable matters through the influence of the air and ammoniacal salts derived from decomposing animal manures and from rain. It forms from the crenates and humates, when they are exposed to atmospheric influence, by breaking up the soil during cultivation. Hence we see that a yellow soil turns black by two or three years' exposure to the atmosphere, and from an unfertile state becomes fertile. We can readily produce the same result in a few hours, when we operate on a small quantity of soil in the laboratory. Apocrenic acid forms salts with lime and with peroxide of iron, which are nearly insoluble in water, but which are readily decomposed by the action of carbonate of ammonia, or by potash or soda; so that we may readily conceive of the fertilizing influence of these alkaline matters, since they render the organic manures, which were before insoluble, perfectly soluble in water, so that they may be absorbed by the rootlets of plants. If these matters are absorbed, as they infallibly must be, it is evident that they must undergo a series of modifications in the sap vessels, so that they are no longer found in the juices of plants in the state which they were when they were first absorbed. Now, by analysis of the sap of plants before the putting forth of their foliage, we find certain extractive matters and sugar. In some, apoglucic and glucic acid have been discovered.

Let us then consider the composition of these substances, and see whether it is possible for humic acid, for instance, to pass by chemical changes into sugar.

	Carbon.	Hydrogen.	Oxygen.
Sugar is composed of - - -	6	10	5
Glucic acid of - - -	8	10	5
Humic acid of - - -	40	24	12
Ulmin from sugar - - -	40	32	14

* The erroneous statement in the Amer. Jour. of Science, was owing to the reviewer having only a spurious edition of Berzelius' Chemistry, which was printed in French at Bruxelles.

Now 11 atoms of sugar consists of 66 carbon, 110 hydrogen, 55 oxygen, and may form, by decomposition according to Mulden,

	Carbon.	Hydrogen.	Oxygen.
3 atoms of glucic acid, - - -	24	30	15
1 atom of ulmin, - - -	40	32	14
1 atom of formic acid, - -	2	2	3
23 atoms of water, - - -	0	46	23
	66	110	55

Then by the opposite action, which is the regeneration of vegetable matter, we may have ulmin or humic acid converted into glucic acid, formic acid or sugar.

By the action of the atmosphere, glucic acid is converted into apoglucic acid, and by the action of nitrogenized bodies it is further converted into crenic acid and apocrenic acid. Thus we see how these important organic matters may be formed from ordinary vegetable matter, by exposure to the influence of moisture, atmospheric air, and the soil.

Dumas regards vegetables as true organs of reduction or deoxidation, while animals are regarded as organs of combustion or oxidation. Thus while plants absorb carbonic acid and give out oxygen, animals absorb oxygen and give out carbonic acid gas. Plants directly or indirectly furnish us with our aliment, and after consuming it, animals render the same amount of matter to the vegetable kingdom in another form, which is better adapted to their use. Thus we see the great and mysterious circle of the revolution of organic elements is complete.

Besides the organic matters derived from vegetable mould, we must consider the action of saline matters which constitute the necessary components of plants.

We have already seen by the beautiful researches of Prof. Bailey, that nearly all kinds of forest trees contain in their bark, and enclosed in their sap-vessels, crystals of oxalate of lime, more than a million of which have been discovered in a square inch of the bark of the birch tree. This discovery is truly interesting, for it shows us another step in the process of changes of organic matter; for it is one of the easiest things for the chemist to convert sugar into oxalic acid. We must not fail to remark also, that while oxalic acid is a deadly poison, oxalate of lime is perfectly harmless; so that here we have this powerful acid in chains. What function this salt performs in vegetation is yet unknown.

When we examine any kind of grain or any seed which is not charged with oil so as to prevent an examination by the test we shall now describe, it is discovered that the seeds contain phosphates of certain bases. During the spring of 1840, Dr. J. discovered the presence of

phosphoric acid in Indian corn, wheat, oats, beans, peas and chestnuts. This was done by burning away the seeds by heat and nitric acid, when in several cases, glacial phosphoric acid and phosphates of lime and magnesia were discovered. In his first operations, the whole seed was burnt in order to obtain the ashes for analysis, but having been shown by Mr. A. A. Hayes, specimens of Indian corn which were soaked in a weak solution of sulphate of copper, whereby the precise limits of the phosphates were shown by the formation of phosphates of copper, he was induced to examine the situation of these matters by this test; and then by dissecting out the organs of the plant which contained the phosphates, he was enabled to analyze them in a more thorough manner, so as to arrive at interesting and important results.

Specimens of various seeds were exhibited to the Association, which demonstrated the presence of phosphates in the cotyledons only. This was stated to be a general fact in every case where the experiment had been tried. The presence of the salts above noticed is a most important discovery. It explains the origin of the bones of animals.

Around the cotyledon of Indian corn, Mr. Hayes discovered a layer of a salt of peroxide of iron. This was also demonstrated by examples shown to the Association. This iron shield around the cotyledon of corn is not to be overlooked, for it is the source of the oxide of iron which enters into the composition of the red globules of the blood of animals.

Indian corn also contains a fat oil which exists in the transparent hard portion of the corn, combined with starch and a peculiar nitrogenized body called zeine. This serves to form the fat of animals, and the starch and zeine form the carbonaceous compounds of the muscles and tissues.

Dr. L. C. Beck then read a paper from J. N. Nicollet, on the mineral region of the state of Missouri.

The mineral region embraced in this paper, comprises the Valleé Mines; Mines La Motte; the Pilot Knob; and the Iron Mountain. At Valleé's mines the principal ore is the common sulphuret, (galena,) which is invariably found in ferruginous clay containing concretions of argillaceous oxide of iron, and lumps of the radiated and crystalline varieties of sulphuret of iron. The *lead-bearing clay* is at irregular depths from the surface, and is of variable thickness; under it is a limestone. The main shaft of this mine is sunk about two hundred feet, and the lode is computed to be about four hundred feet above the Mississippi, the level of the country being six hundred feet above the same.

The following section furnished by the superintendent of the mines, shows the descending order and thickness of the several beds.

1.	Ferruginous clay mixed with rubble, - -	30 feet.
2.	Red clay mixed with large masses of rock requiring blasting, - - - - - - -	130 "
3.	Red clay similar to No. 1, in the seams of lead,	70 "
4.	Rock masses of limestone, - - - -	4 "
5.	Lode, in red clay, of indifferent thickness, say	40 "
		234 "

Pursuing a general direction, at the bottom of the shaft, the ore is found to swell out laterally into pouches, or branches off so as to admit of lateral openings; so that the whole extent of the galleries is estimated at fifteen miles under ground. Over the surface, the lead region may be distinctly traced.

Tuesday morning, May 5.—The secretary read a paper by Mr. Nicollet, on the earthquake of New Madrid.

Prof. H. D. Rogers communicated to the Association the results of his researches in relation to the recent earthquakes, and gave an outline of a theory of earthquake action, by which he and his brother Prof. W. B. Rogers propose to explain the forces concerned in the formation of anticlinal flexures, and to account for several other dynamic phenomena in geology.

The characteristic features of earthquake motion, were shown to consist, as originally stated by Michell, in a peculiar rapid undulation or wave-like rocking of the ground, and a short vibratory jar or tremor, the tremulous shaking seldom extending to as great a distance from the source of the earthquake as the undulation. Details of several earthquakes were quoted, to prove that the rocking motion is a true billowy undulation. Thus, during the severe shock so destructive at Hayti in May, 1842, the public square at Porto Plata was seen to undulate like the waves of the sea, the houses rocking to and fro like vessels in a storm. In the great earthquake which shook the Windward Islands on the 8th of February last, the earth in Antigua reeled to and fro for more than two minutes, and the same is mentioned in relation to the motion at Guadaloupe and every other locality, where the phenomena were carefully noted.

The captain of the British steamer Tay reports, that being off Antigua and looking towards the shore, *the hills appeared to be in motion.* During the earthquake of Conception in 1835, the motion of the ground

at Valdavia, according to the interesting description of Capt. Fitzroy, was undulating and regular, like waves rolling, from west to east, but strong; and it lasted nearly ten minutes. The houses waved and cracked. The same earthquake was felt at the island of Chiloe by Mr. Douglas, who describes the motion, we are told by Mr. Darwin, as horizontal and slow, similar to that of a ship at sea, going before a high regular swell, with three to five shocks in a minute.

Prof. Rogers proceeded to show the manner in which these earthquake undulations advance, and adduced some facts from which even the amplitude of the individual waves or pulsations may be approximately computed. As a confirmation of the truth of the generalization, long ago arrived at by Michell, that the disturbance is not simultaneous over the whole region shaken, but is transmitted with a high velocity, he presented the results of an analysis of the earthquake which happened in the United States, on the 4th of January of the present year, respecting which he had collected some instructive information.

This earthquake was felt from the sea-coast of Georgia and South Carolina, to beyond the western frontier military posts, and from the latitude of Natchez to that of Iowa, a space in each direction of about eight hundred miles; and there are reasons for believing that its actual extent was considerably greater. A comparison of the dates of the shock, as felt at the numerous localities heard from, seems to settle with satisfactory accuracy, the *direction*, *velocity* and *mode of progress* of this earthquake. The facts collected from more than twenty five stations, and embodied in a tabular form, make it obvious that the shock was simultaneous throughout an elongated and narrow belt or line, ranging in a N. N. E. direction from the western edge of Alabama, nearly through Nashville and Cincinnati, being also simultaneous along every other line having with this a parallel direction; whereas no such synchronism existed, where the localities compared were situated in any other than a N. N. E. and S. S. W. position. Places lying to the W. N. W. of others invariably encountered the convulsion soonest, and by an interval of time, in this case, *strictly* proportionate to the distance.

These general facts justify, it is conceived, two important conclusions; first, that the area in agitation at any given instant was *linear*, and secondly, that the earthquake moved from W. N. W. to E. S. E., keeping parallel to itself in the manner of an advancing wave. The data brought together in the table, indicate with considerable precision the *velocity* with which the shock was propagated from W. N. W. to E. S. E. Ascertaining the moment when the belt of synchronal disturbance reached St. Louis, and that again when it coincided with a line passing through Tuscaloosa, Nashville and Cincinnati, the earthquake is found to have occupied about eight minutes and twenty four seconds in the transit, the

distance passed over being about two hundred and seventy miles. From this it would appear that the velocity was about thirty two miles per minute. If in like manner the times be compared when the earthquake was at Nashville, and again in a mean position between Columbia and Charleston, it will be found to have occupied in this part of its rapid march, about eleven minutes and eighteen seconds, but the distance being three hundred and eighty miles, the velocity indicated was nearly thirty three and a half miles per minute. So close an agreement of the two sets of results, bespeaks the accuracy of the data upon which these computations are based.

In support of the above inferences respecting the direction of transmission of the earthquake, is the observed direction of the oscillating motion, which is described as having been at various localities from west to east.

The phenomena of the recent earthquake of Guadaloupe were next alluded to, as confirming in a remarkable manner the accuracy of the general laws of earthquake motion, deduced from the above described earthquake of the United States. In the West India convulsion, the velocity of transmission was about twenty seven miles per minute, the pulsation being propagated laterally from an immensely elongated axis of disturbance, extending in a N. and S. direction, through the Windward Islands, to Bermuda on the N., and to the coast of Guiana on the S.

Respecting the origin of the wave-like motion of the ground in earthquakes, some able writers have been disposed to consider it as the result of a mere tremulous jar radiated from some deep-seated focus, or line of sudden fracture, and reaching the surface at points more and more remote from the source of disturbance. All the circumstantial descriptions of the phenomena disclose, however, an essential and characteristic difference in the two motions, and plainly indicate the wave-like motion to be an actual billowy oscillation of the earth's crust. That the vibratory jar is not the cause, but itself the necessary consequence of the undulation, is apparent from the following considerations. A mere tremulous vibration transmitted along a given column through the earth's crust, would not sensibly elevate or depress the surface, since the waves of compression and dilatation among the particles of the column, would neutralize each other in their effect on the dimensions of the mass. It is difficult to conceive, moreover, how the broad, ample and comparatively slow pulsations of the earthquake can by any cumulative process, be the result of those almost infinitely more minute and rapid waves which constitute vibration in solids, and which in earthquakes are the tremulous jar, and the cause of the characteristic rumbling sound. But if it were even practicable to account for the enormous magnitude of the low, broad waves into which the crust is thrown,

how shall we derive from a vibration radiated from a deep focal spot or line, their remarkable *number* and *isochronism?* A simple vibratory jar sent through the crust, if competent to produce a great wave at all, could on the hypothesis produce no more than *one*, as the result of a single concussive force, so that when these waves follow each other, at regular intervals of a quarter of a minute or more, for several minutes, we must admit that they are generated in some other manner, since there is no conceivable cause for a strictly isochronous repetition of the subterranean force.

But an objection of another kind suggests itself, in the excessively fissured and crushed condition of the strata in many regions, and their extremely heterogeneous composition, which must inevitably lead to a rapid dispersion or breaking up of all regular waves of vibration within the rocky mass.

An eminent British geologist has suggested that the undulatory motion in earthquakes, may be of the nature of the vibration in a stretched cord when it is struck; but Prof. Rogers and his brother find it difficult to imagine, if we deny the theory of a pulsating fluid under the crust, how in a mass so little homogeneous and so unelastic, nodal vibrations could take place in the solid fabric of the globe, causing waves of the height and amplitude of the earthquake undulations.

Rejecting the opinion that the vibratory jar is the cause of the undulatory motion, they deem it more in accordance with known phenomena, to recognize it as the *effect*, and to attribute the tremor to an extensive, minute fissuring and grinding together of the strata under the alternate dilatation and compression going on in every part of the rocky mass, during the undulation.

The dimensions of the individual inundations would appear to be susceptible in certain cases of direct calculation. Though the waves or temporary flexures must be of various magnitudes in different earthquakes, their amplitude in the more violent convulsions is manifestly very great. Thus taking the data furnished by Darwin in his account of the earthquake of Conception, it may be shown that the probable width of each pulsation in that instance, was at least ten geographical miles, while there is reason to conclude that in the great Lisbon earthquake, each wave of the crust had an amplitude amounting to twenty five miles.

That the wave-like motion of the earth is of the character of an actual billowy pulsation, is shown not only by the visible heaving of the ground, but by the sensations produced, and by the alternate opening and closing of enormous parallel chasms of great depth, and the direction of these, which is perpendicular to the course of the undulation. Of the manner in which the wave-like movement in earthquakes may

be supposed to originate, Michell suggests, that large tracts of country may be supposed to rest on fluid lava, which when disturbed may transmit its motion through the overlying crust. Comparing the process to the waves caused in a carpet, when one end of it is lifted from the floor and suddenly brought down again, he conceives, that "a large quantity of vapor may raise the earth in a wave, as it passes along between the strata, which it may readily separate in a horizontal direction, there being little or no cohesion between one stratum and another. The part of the earth that is first raised, being bent from its natural form, will endeavor to restore itself by its elasticity, and the parts next to it having their weight supported by the vapor which will insinuate itself under them, will be raised in their turn, till it either finds some vent or is again condensed by the cold into water, and by that means prevented from proceeding any further."

Prof. Rogers and his brother propose an explanation of the origin of the pulsation which they deem more in harmony with sound dynamic considerations, and with the observed phenomena of earthquakes. Instead of supposing it possible for a body of vapor to pass horizontally between the strata, or even between the crust and the fluid lava, which at their contact must be closely entangled, they attribute the movement to an *actual pulsation* in the molten matter itself, engendered by a linear disruption of the crust from enormous tension, and the sudden or explosive escape of highly compressed steam and gaseous matter. Upon this doctrine the course of the subterranean vapors would be towards and not from the line of disruption, and the undulation of the crust would arise from the instantaneous and excessive change in the pressure on the surface of the lava mass, the operation of which would be as effectual as a sudden downward stroke in creating in the fluid a system of great oscillatory waves. The billows excited on the surface of the sea of molten lava, by the rupturing and immediate collapsing of the crust, must, it is conceived, be of the nature of progressive waves of oscillation. Generated in a group on each side of the axis of disturbance, these waves will move off in parallel order, the two belts coalescing at their extremities to form a rapidly dilating *elliptic* zone, the outline of which will mainly depend on the form and elongation of the rent. Around the extremities of the fissure, the pulsation will be feeble from the rapid *radiant* progress, in this position of the waves, and this perhaps may explain the absence of a sensible shock during the Guadaloupe earthquake, in the region N. and N. W. of Bermuda, while it was distinctly felt to a great distance in a due west direction. If the earth's crust be ruptured along a very short line, or the rent be by the orifice of a volcano, the pulsation will be approximately *circular*. Such seems to have been nearly the form of the celebrated Lisbon earthquake.

Should the line of disruption, on the other hand, be greatly elongated, and the pulsations on *one side* of it only be studied, the belt of advancing waves may seem *straight*, the apparent form of the lines of synchronous shock in the recent Mississippi earthquake.

The views here suggested of the nature and cause of the wave-like motion in earthquakes, rest upon a generalization which the authors of this communication regard as one of the soundest deductions in geology, that fluid lava underlies large regions of the earth's crust, and that this crust is of very moderate thickness. If it be conceded that the earthquake undulation and its attendant phenomena imply actual pulsation in a subjacent fluid, the whole tendency of geological fact is to demonstrate that this fluid can be only intensely heated rock or lava. And conversely, the frequent recurrence of earthquakes in every known district of the globe, and the vast distances over which these pulsations are transmitted,—in some instances more than three thousand miles,—are facts which lend strong support to the doctrine of central heat, since they indicate that the internal igneous fluid is absolutely universal.

Prof. Rogers next gave a concise description of the structural features of the great Appalachian chain of the United States, illustrating the nature of the flexures in the strata, their remarkable parallelism and great length, their distribution in groups, and the law of their successively diminishing curvation, crossing the region from southeast to northwest. Referring to the published volume of the Transactions of the Association, for a full exposition of the views of his brother and himself in explanation of the phenomena, he confined himself to shewing that the bending and elevation of strata in regular flexures or axes, is the necessary consequence of a wave-like oscillation of the crust, acting simultaneously with a horizontal or tangential pressure. The identity of the ancient undulations, thus causing permanent flexures, with modern earthquakes, was then maintained, and facts appealed to in proof that these convulsions sometimes produce permanent anticlinal arches of gentle curvature, at the present day.

Other applications of the theory of the paroxysmal undulation of the earth's crust, were then adverted to; particularly the ready explanation it affords of the remarkably wide and uniform distribution of the coarse materials in some of our rocks of mechanical origin. It was argued, that to no aqueous action less extensive than that of an inundation as broad and diffused as an earthquake, can we attribute the strewing of the great sheets of matter now forming certain conglomerates and sandstones. Repeated oscillations of the crust, if very vehement, and accompanied as they would be by some permanent elevation of the surface, might send the ocean upon the dry land, and form of the fragmentary detritus a superficial layer as broad as the area inundated.

The amazing momentum imparted by the pulsating crust to the sea above it, and the huge magnitude of the surges into which this would be thrown, would contribute greatly to the dispersion of the erratic matter. In this manner Prof. Rogers conceives we may explain, by a series of tremendous earthquakes proceeding from a high arctic latitude, the rush of mingled water, ice, and fragmentary rock, at the formation of the so called "drift" which now overspreads all the northern parts of North America and Europe. Upon this view of the origin of that interesting stratum, a competent cause is suggested for the almost *universal* scratching and polishing of the floor upon which the drift reposes, since the enormous velocity that would be imparted by the vast sea-waves to successive portions of the angular rocky matter, would naturally give the blocks and fragments great cutting power. It seems unnecessary therefore to appeal to the stranding of icebergs, for the force which scored and grooved the surface of the strata throughout the northern latitudes.

Dr. Jackson exhibited to the members several specimens of sulphuret of antimony, silver ore, blende, and the tin reduced from the ore, exhibited at the last meeting, all from the state of New Hampshire.

The attention of the meeting was then called to several points of business from the standing committee.

The following nominations for membership, were proposed and confirmed. Prof. Albert Hopkins, Schenectady, N. Y., Mr. W. J. Lettsom, British legation, Washington, D. C., Dr. Norwood, Madison, N. Y., Dr. A. Clapp, New Albany, Ind., Dr. Charles Pickering, Washington, D. C., Hon. Judge B. Tappan, Steubenville, Ohio, Mr. M. Tuomey, Petersburg, Va., Dr. J. T. Plummer, Richmond, Indiana, Erastus Smith, Esq., Hartford, Ct., Dr. Jeffries Wyman, Boston, Rev. E. H. Newton, Cambridge, N. Y., Mr. Geo. Gibbs, Mr. Wolcot Gibbs, and Dr. J. R. Chilton, New York, Prof. D. Olmsted, New Haven, Ct., Dr. Wm. M. Carpenter, and Dr. J. L. Riddell, New Orleans, Mr. Root, Syracuse, N. Y., Dr. Charles Page and Hon. H. L. Ellsworth, Washington, D. C., Lt. Ruggles and Col. H. Whiting, Detroit, Mich., Mr. J. G. Anthony, Cincinnati, Ohio, Dr. William Darlington, Westchester, Pa., Mr. R. Buchanan, Cincinnati, Ohio, Dr. J. P. Kirtland, Geo. H. Cooke, Dr. John Wright, Troy, N. Y.

Resolved, That all papers read at the present session of this Association, be handed as far as practicable to the Secretary, before the rising of the Association.

Resolved, That at all future sessions of this Association, the various papers read, and reports submitted to the session, shall be considered, from the time of their presentation, as the property of the Association, and shall be delivered by the author to the secretary at the time.

Resolved, That the several committees appointed at the last session of the Association, which have not reported or have not been remodeled, be continued and their several members be requested to report at the next session.

Resolved, That this Association hold its next annual meeting at the city of Washington, on the second Wednesday of May, 1844.

The following were the officers elected for the next meeting of the Association.

Chairman.—Dr. John Locke, Cincinnati, Ohio.

Secretary.—Dr. David Dale Owen, New Harmony, Indiana.

Treasurer.—Dr. Douglass Houghton, Detroit, Michigan.

Local Committee.—Hon. H. L. Ellsworth, Prof. F. Hall,* Francis Markoe, Jr., Dr. Chas. Pickering, J. D. Dana.

Standing Committee.—Same as last year, viz. Prof. E. Hitchcock, Dr. Ducatel, Dr. C. T. Jackson, Dr. L. C. Beck, L. Vanuxem, Dr. J. B. Rogers, Prof. J. W. Bailey, Prof. B. Silliman, John L. Hayes, Esq.

Resolved, That hereafter the chairman of each meeting be elected at the meeting over which he is to preside.

Resolved, That the following gentlemen be requested to prepare reports on the present state of our knowledge on the various subjects affixed to their several names, and as far as practicable, to present them to the Association at its next session.

On Fossil Corals.—James D. Dana, Dr. A. Clapp, and John Gebhard, Jr.

On Fossil Foot-marks.—Prof. E. Hitchcock.

On Fossil Crustacea and Crinoidea.—James Hall.

On American Fossil Botany.—H. D. Rogers, J. E. Teschemacher, and J. W. Bailey on the microscopic portion.

On American Forest Trees and their distribution.—Geo. B. Emerson.

On American Cryptogamia.—Rev. Mr. Russel, Mr. Edward Tuckerman, Jr.

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* Since deceased.

On Entozoa.—Dr. J. Wyman.

On the Fossil Osteology of North America.—Dr. J. Wyman.

On the Geological distribution of Minerals.—J. D. Dana.

On the Chemical relations of the American Coals.—W. B. and R. E. Rogers.

On the Chemical and Economical Relations of the Green Sand of the United States.—J. B. Rogers and C. T. Jackson.

On the Native Compounds of Lime, Magnesia, Iron and Manganese.—Martin H. Boyé and James C. Booth.

On the Evaporating Power of various Coals.—W. R. Johnson.

On the Comparative Ichthyology of the Coast of North America and Europe.—D. H. Storer.

On the Fossil Fishes of the United States.—John H. Redfield.

On Volcanic Phenomena and the Distribution of Volcanoes.—J. L. Hayes.

On Drift Phenomena.—Prof. E. Emmons, for the New England states and New York; J. N. Nicollet,* for the West and far West; W. B. Rogers, for the Southern boundary.

Afternoon.—Mr. Hall read a paper upon the Crinoidea of the rocks of New York, their geological and geographical distribution.

These may be regarded as the most singular and beautiful fossils of our older rocks. When found in all their gorgeous perfection, they remind one of the fanciful creations of some fairy tale; and the glowing descriptions of the coral groves of our tropical seas can be in some degree appreciated.

If we find their external and general characters beautiful, their more minute and intricate structure is often still more curious and interesting.

Mr. H. referred to the ingenious work of Miller, published in England in 1821, as the groundwork on which we are enabled to found generic distinctions. Mr. Say's descriptions of the Caryocrinus ornatus and C. loricatus are almost the only scientific descriptions of Crinoidea with which I have met. Some notices and figures of other species from different parts of the country, serve to show how rich are our rocks in these remains.

Here follows a short description of the general structure and habits of these animals. In some species the mouth, composed of small triangular plates, shows a close analogy with the Echinidea. The delicate structure of this class of animals, rendered them peculiarly liable to destruction upon slight causes; and their immense numbers are only

* Since deceased, Sept. 11, 1843.

attested by their comminuted remains, which in some places constitute thick masses of limestone. They are likewise scattered throughout the calcareous shales, and commenced their existence at a very early period in the earth's history, and their remains are found in greater or less abundance, from the calciferous sandrock through all the formations of the New York system.

In the lower rocks, these remains, with one or two exceptions, are so far obliterated as not to be referable to their appropriate genera. It is not until we reach the rocks of the Niagara group, that we find them in any degree of perfection. In the finely comminuted homogeneous mud deposit forming the lower member of this group, these animals flourished in great numbers and equal perfection. There are in this group more ascertained species than in all the rocks of New York besides, no less than nine being already established. Several of these are of genera unknown elsewhere, and are therefore interesting from their unique character.

Here followed a description with figures and specimens illustrating the structure of the Caryocrinus ornatus, the Hypanthocrinites decorus and H. cœlatus, the Cyathocrinites pyriformis, and several other species. Some of these were exceedingly beautiful and delicate in their structure. Several other forms from the Hamilton, Portage and Chemung groups were also exhibited, some of them of great beauty and perfection.

In their geological range, these fossils appear to be more limited than any other forms; and so far as my observation extends, not a single form is known to extend beyond the rock or group of which it is typical. Their fragile nature appears to have been such, that they were unable to withstand any great changes in the condition of the ocean bed, and when a quiet deposit is succeeded by one made in a more disturbed sea, they seem all to have perished. This was shown in the Niagara group, in the transition from the calcareous mud to limestone.

In all the distinct masses forming the Helderberg series of limestones, we find these remains to change with the commencement of a new deposit; and though often only fragments occur, they are yet sufficiently characteristic. When the shales of the Hamilton group succeed these limestones, we find a new and distinct creation of these animals, differing from those of the rocks below. Among these are many beautiful forms, though they are for the most part imperfect. In this group commences the Pentremite, and an allied genus, the Nucleocrinus.

In the Portage and Chemung groups of rocks, these remains are likewise entirely different from any of those below, and different from each other. Though the materials of the two groups are similar, there are no remains continued from the lower to the higher.

Geographical distribution.—From the extensive destruction of these animals, and the transportation of their broken remains over large areas, it is impossible to determine what has been their original extent when living. The fragments have doubtless been often drifted to great distances from their original places of growth. Wherever they are known to exist in any degree of perfection, their geographical extent is very limited. Of the nine or ten species known in the Niagara group, not more than one is known to range beyond a distance of forty or fifty miles, and this one not more than sixty or seventy. Nearly all of them are confined to the space of a few miles. Certain situations appear to have been favorable to their growth, and though the nature of the deposit may appear equally uniform for a much greater distance, they do not occur.

In the limestones of the Helderberg series, their remains are widely distributed, but from their being usually in fragments, the fact furnishes no argument that they were thus widely distributed when living. In most cases in this series and in the Hamilton group, the perfect specimens are scarcely known beyond the single locality.

In the Portage group, the only perfect species known does not extend horizontally more than ten or twelve feet, and the place appears as if a forest of these beautiful forms had been swept down and covered with the soft mud above.

The well ascertained species of the Chemung group, are almost equally limited in their geographical range.

From all the facts collected it appears, that certain species, though preëminently typical of certain formations, cannot be relied upon over any wide area of country. Their presence may be relied upon as identifying certain formations, and their absence is not by any means to interfere with conclusions regarding identity drawn from other sources.

The natural history of this class of animals, in connection with their geological distribution, is exceedingly curious and interesting, and these few facts may serve to enlist observation upon their situation and condition in other parts of the country.

Dr. Owen read a paper on a universal system of geological coloring and symbols.

It was proposed that the three primitive colors should be adopted to represent the three great palæontological periods, viz. blue (indigo) for the primary fossiliferous; red (light red, or a carmine tint) for the secondary; yellow (gamboge) for the tertiary,—that the principal groups of these periods, when compatible with distinctness, be indicated by a variation in the tint, the intensity increasing in the descending order, and, if necessary, imparting at the same time to the limestones a bluish cast, and to the argillaceous deposits a greyish tint.

Pink (lake) was proposed for granitic rocks—greens for serpentine, greenstone and trap; the first being the light green, the last the darkest; or if particular greens for each be thought preferable, *terre verte* may represent serpentine; *sap green*, greenstone; *Prussian green*, trap; *reddish grey*, for trachyte; *light grey*, for modern lavas; *reds* with *yellow spots*, for porphyry; *grey* with *white spots*, amygdaloid. A *purple* color composed of the lake of the granite and the indigo of the lowest fossiliferous, was thought to be the most appropriate color for the metamorphic rocks. If it should be thought desirable to distinguish the different members of the metamorphic group, *neutral tint* might represent gneiss; *native ochrous purple*, mica slate; *archal*, hornblende schist; and the mixture of *indigo* and *lake* of the period clay slate and killas. *Ultramarine*, or *ultramarine* and *lake*, might represent metamorphic limestone. For the carboniferous rocks, considered by some an independent formation, *burnt sienna* was recommended, which having a decidedly red hue, would give to this formation the tint of the period to which it belongs.

In detailed sections and charts the same general system should be adhered to. If a variation of the tint should not be found sufficiently distinct in such cases, then sandstones and siliceous deposits of the first period may uniformly be indicated by *chrome yellow;* those of the second period, by *yellow ochre* or the red color of the period; those of the third period, by *gamboge yellow;* limestones of the first period may be represented by *indigo;* those of the second period, by *Prussian blue;* those of the third period, by *cobalt blue.* If possible, the demi-tints should not predominate, but rather the primitive color of the period.

All the primitive tints being transparent colors, they can be more easily laid on in a neat, delicate and uniform manner, than the opaque mineral pigments; and, if desirable, they may be glazed on the top of the lithological tints, and the general hue of the period thus imparted.

Thus, according to the plan proposed for detailed geological sections, blue will always indicate limestones or calcareous deposits; greys, argillaceous strata; greens, argillo-calcareous deposits; dark grey or black, carbonaceous beds.

In addition to this system of coloring, still further to facilitate the recognition of formations, a system of symbols was also proposed, on a similar plan to chemical symbols, taking the first letter of the name of a formation to represent it, and, when that was appropriated, the first and second, or when two or three words are employed, the first letter of each word, thus:

Granite, G.	Gneiss, Gn.
Metamorphic rocks, M.	Mica slate, M. S.

Hornblende schist, H. S.	Green sand, G. S.
Metamorphic limestone, M. L.	Chalk, Ch.
Chlorite schist, Ch. S.	Lower cretaceous, L. Cr.
Clay slate, C. S.	Middle cretaceous, M. Cr.
Cambrian rocks, C.	Upper cretaceous, U. Cr.
Silurian, S.	Eocene, E.
Old red sandstone, O. R. S.	Miocene, Mi.
Mountain limestone, Mn. L.	Older pliocene, O. P.
Millstone grit, M. G.	New pliocene, N. P.
Carboniferous rocks, Ca.	Post pliocene, P. P.
Red conglomerate, R. C.	Serpentine, Se.
Magnesian limestone, Ma. L.	Greenstone, Gr.
Zechstein, Z.	Trap, T.
New red sandstone, N. R. S.	Basalt, B.
Lias, L.	Trachyte, Tr.
Inferior oolite, I. O.	Porphyry, P.
Middle oolite, M. O.	Amygdaloid, Am.
Upper oolite, U. O.	Lava, La.
Weald, W.	

A paper was then read by Mr. Nicollet, on the mineral resources of St. Louis and its vicinity. It was then

Resolved, That this Association close its present session and adjourn te meet on the second Wednesday of May, 1844, at 10 o'clock, A. M., at Washington, D. C.

H. D. Rogers, *Chairman.*

B. Silliman, Jr. *Secretary.*

Art. XI.—*On the upright Fossil Trees found at different levels in the Coal Strata of Cumberland, Nova Scotia;* by Charles Lyell, Esq., F. G. S., F. R. S., &c.

[Communicated to this Journal by the author.]

The first notice of these fossil trees was published in 1829 by Mr. Richard Brown, in Haliburton's Nova Scotia, at which time the erect trunks are described as extending through one bed of sandstone twelve feet thick. Their fossilization was attributed by Mr. Brown to the inundation of the ground on which the forest stood. Mr. Lyell in 1842 saw similar upright trees at more than ten different levels, all placed at right angles to the planes of stratification, which are inclined at an angle of 24° to the S. S. W.

The fossil trees extend over a space of from two to three miles from N. to S., and according to Dr. Gesner, to more than twice that distance from E. to W. The containing strata resemble lithologically the English coal-measures, being composed of white and brown sandstones, bituminous shales, and clay with ironstone. There are about nineteen seams of coal, the most considerable being four feet thick. The place where these are best seen is called the South Joggins, where the cliffs are from one hundred and fifty to two hundred feet high, forming the southern shore of a branch of the Bay of Fundy, called Chignecto Bay. The action of tides, which rise sixty feet, exposes continually a fresh section, and every year different sets of trees are seen in the face of the cliffs.

The beds with which the coal and erect trees are associated are not interrupted by faults. They are more than two thousand feet thick and range for nearly two miles along the coast. Immediately below them are blue grits, used for grindstones, after which there is a break in the section for three miles, when there appear, near Minudie, beds of gypsum and limestone, and at that village a deep red sandstone, the whole having the same southerly dip as the coal at the Joggins, and being considered by Mr. Lyell as the older member of the carboniferous series.

Above the coal-bearing beds and stretching southward for many miles continuously along the shore, are grits and shales of prodigious thickness, with coal plants, but without vertical trees.

Mr. Lyell next describes in detail the position and structure of the upright trees at the South Joggins. He states that no part of the original tree is preserved except the bark, which is marked externally with irregular longitudinal ridges and furrows without any leaf-scars, precisely resembling in this respect the vertical trees found at Dixonfold, on the Bolton Railway, described by Messrs. Hawkshaw and Bowman. No traces of structure could be detected in the internal cylinder of the fossil trunks, which are now filled with sandstone and shale, through which fern leaves and other plants are occasionally scattered. Mr. Lyell saw seventeen vertical trees, varying in height from six to twenty feet, and from fourteen inches to four feet in diameter. The beds which inclose the fossil trees are usually separated from each other by masses of shale and sandstone, many yards in thickness.

The trunks of the trees, which are all broken off abruptly at the top, extend through different strata, but were never seen to penetrate a seam of coal however thin. They all end downwards, either in beds of coal or shale, no instance occurring of their termination in sandstone. Sometimes the strata of shale, sandstone and clay with which the fossil trunks have been filled are much more numerous than the beds which they traverse. In one case nine distinct deposits were seen in the interior of a tree, while only three occurred on the outside in the same vertical height.

Immediately above the uppermost coal-seams and vertical trees are two strata, probably of fresh-water origin, of black calcareo-bituminous shale, chiefly made up of compressed shells of two species of Modiola and two kinds of Cypris.

Stigmariæ are abundant in the clays and argillaceous sandstones, often with their leaves attached, and spreading regularly in all directions from the stem. The other plants dispersed through the shales and sandstones bear a striking resemblance to those of the European coal-fields. Among these are Pecopteris Conchitica, Neuropteris flexuosa? Calamites cannæformis, C. approximatus, C. Steinhaueri, and C. Nodosus, Sigillaria undulata and another species. The genera Lepidodendron and Sternbergia are also present. The same plants occur at Pictou, and at Sydney in Cape Breton, accompanied with Trigonocarpum, Asterophyllites, Sphænophyllum, and other well known coal-fossils.

The author then gives a brief description of a bed of erect Calamites, first discovered by Mr. J. Dawson, in the Pictou coal-field, about one hundred miles eastward of the Cumberland coal-measures before described. They occur at Dickson's mills, one mile and a quarter west of Pictou, in a bed of sandstone about ten feet thick. They all terminate downwards at the same level, where the sandstone rests on subjacent limestone, but the tops are broken off at different heights, and Mr. Dawson observed in the same bed a prostrate Lepidodendron with leaves and Lepidostrobi attached to its branches.

From the facts above enumerated Mr. Lyell draws the following conclusions:—

1. That the erect position of the trees, and their perpendicularity to the places of stratification, imply that a thickness of several thousand feet of coal-strata, now uniformly inclined at an angle of 24°, were deposited originally in a horizontal position.

2. There must have been repeated sinkings of the dry land to allow of the growth of more than ten forests of fossil trees one above the other, an inference which is borne out by the independent evidence afforded by the Stigmaria found in the under-clays beneath coal-seams in Nova Scotia, as first noticed in South Wales by Mr. Logan.

3. The correspondence in general characters of the erect trees of Nova Scotia with those found near Manchester, leads to the opinion that this tribe of plants may have been enabled by the strength of its large roots to withstand the power of waves and currents much more effectually than the Lepidodendra and Sigillariæ, which are more rarely found to retain a perpendicular position.

Lastly, it has been objected that if seams of pure coal were formed on the ground where the vegetables grew, they would not bear so precise a resemblance to ordinary subaqueous strata, but ought to undulate like the present surface of the dry land. In answer to this Mr. Lyell points to what were undoubtedly terrestrial surfaces at the South Joggins, now represented by coal-seams or layers of shale supporting erect trees, and yet these surfaces conform as correctly to the general planes of stratification as those of any other strata.

He also shows that such an absence of superficial inequalities and such a parallelism of successive surfaces of dry land, ought to be expected according to the theory of repeated subsidence, because sedimentary deposition would continually exert its levelling action on the district submerged.

ART. XII.—*On the Coal Formation of Nova Scotia, and on the Age and Relative Position of the Gypsum and accompanying Marine Limestones;* by C. LYELL, Esq. F. G. S. &c. &c.

[Communicated to this Journal by the author.]

THE stratified rocks of Nova Scotia more ancient than the carboniferous consist chiefly of metamorphic clay-slate and quartzite, their strike being nearly east and west. Towards their northern limits these strata become less crystalline and contain fossils, some of which Mr. Lyell identifies with species of the upper Silurian group, or with the Hamilton group of the New York geologists.

The remaining fossiliferous rocks so far as they are yet known belong to the carboniferous group, and occupy extensive tracts in the northern part of the peninsula, resting unconformably on the preceding series. They may be divided into two principal formations, one of which comprises the productive coal-measures, agreeing precisely with those of Europe in lithological and palæontological character. The other consists chiefly of Red Sandstone and red marl, with subordinate beds of gypsum and marine limestone, but this series is also occasionally associated with coal-grits, shales, and thin seams of coal. A variety of opinions have been entertained respecting the true age of the last mentioned or gypsiferous formation, and it is the purport of this paper to show, first, that it belongs to the carboniferous group; secondly, that it occupies a lower position than the productive coal-measures. These last are of vast thickness in Nova Scotia, being largely developed in Cumberland County, and near Pictou, and recurring again at Sydney in Cape Breton. In all these places they contain shales, probably deposited in a fresh-water estuary, in which several species of Cypris and Modiola abound. The plants of these coal-measures belong to the genera Calamites, Sigillaria, Stigmaria, Lepidodendron, Pecopteris, Neuropteris, Sphenopteris, Nœggerathia, Palmacites, Sternbergia, Sphenophyllum, Asterophyllites, Trigonocarpum, with which are the trunks and wood of coniferous and other trees. Upon the whole nearly fifty species of plants have been detected, more than two-thirds of which are not distinguishable from European species, while the rest agree generically with fossils of the coal formation in Europe.

The internal cylindrical axis of petrified wood in the Stigmaria of Nova Scotia exhibits the same vascular structure, and the same scalariform vessels as the English specimens.

Mr. Lyell next describes the gypsiferous formation, especially the marine limestones of Windsor, Horton, the cliffs bounding the estuary of the Schubenacadie River, the district of Brookfield, and the cliffs at the bridge crossing the Debert River near Truro. Several species of corals and shells are common to all these localities, and recur in similar limestones in Cape Breton. In this assemblage of organic remains we find a Crustacean intermediate between the Trilobite and Limulus, Orthoceras, (two species,) Nautilus, Conularia, Encrinus and Cyathophyllum, besides some species of the carboniferous limestone of Europe, such as Euom-

phalus lævis, Pileopsis vetustus? Pecten plicatus, Isocardia unioniformis, Producta Martini, P. Scotica? Terebratula elongata, Fenestella membranacea? Ceriopora spongites, Goldf. For assistance in determining these the author has been chiefly indebted to M. De Verneuil.

The plants associated with these limestones consist of several species of Lepidodendron, Calamites, and others agreeing with carboniferous forms. With these Mr. Lyell found in Horton Bluff scales of a Ganoid fish, and in the ripple-marked sandstones of the same place Mr. Logan discovered footsteps which appear to Mr. Owen to belong to some unknown species of reptile, constituting the first indications of the reptilian class known in the carboniferous rocks. Several of the shells and corals of this group have been recognized by Messrs. Murchison and De Verneuil as identical with fossils of the gypsiferous deposit of Perm in Russia, and it had been successively proposed, (see Proceedings of the Geological Society, Vol. III, p. 712, and Mr. Murchison's Anniversary Address, Feb. 1843, Vol. IV, p. 125,) to refer these gypsiferous beds of Nova Scotia to the Trias, and to the period of the magnesian limestone. That they are more ancient than both these formations Mr. Lyell infers, not only from their fossils, but also from their occupying a lower position than the productive coal-measures of Nova Scotia and Cape Breton. In proof of this inferiority of position three sections are referred to; first, that of the coast of Cumberland, near Minudie, where beds of red sandstone, gypsum and limestone are seen dipping southwards, or in a direction which would carry them under the productive coal-measures of the South Joggins, which attain a thickness of several miles. Secondly, the section on the East River of Pictou, where the productive coal-measures of the Albion mines repose on a formation of red sandstone including beds of limestone, in which Mr. J. Dawson and the author found Producta Martini and other fossils common to the gypsiferous rocks of Windsor, &c. Some of these limestones are oolitic like those of Windsor, and gypsum occurs near the East River, fourteen miles south of Pictou, so situated as to lead to the presumption that it is an integral part of the inferior red sandstone group. Thirdly, in Cape Breton according to information supplied by Mr. Richard Brown, the gypsiferous formation occupies a considerable tract, consisting of red marl with gypsum and limestone. In specimens of the lat-

ter Mr. Lyell finds the same fossils as those of Windsor, &c. before mentioned. Near Sydney these gypsiferous strata pass beneath a formation of sandstone more than two thousand feet thick, upon which rest conformably the coal-measures of Sydney, dipping to the northeast or seaward, and having a thickness of two thousand feet.

To illustrate the gypsiferous formation the author gives a particular description of the cliffs bordering the Schubenacadie for a distance of fourteen miles from its mouth to Fort Ellis, which he examined in company with Mr. J. W. Dawson and Mr. Duncan. The rocks here consist in great part of soft red marls, with subordinate masses of crystalline gypsum and marine limestones; also three large masses of red sandstone, coal-grits, and shales. The strike of the beds, like that at Windsor, is nearly east and west, and there are numerous faults and flexures. The principal masses of gypsum do not appear to fill rents, but form regular parts of the stratified series, sometimes alternating with limestone and shale.

The author concludes by describing a newer and unconformable red sandstone, without fossils, which is seen to rest on the edges of the carboniferous strata on the Salmon River, six miles above Truro.

ART. XIII.—*On the Microscopic Structure of the Teeth of the Lepidostei, and their Analogies with those of the Labyrinthodonts;* by JEFFRIES WYMAN, M. D.—(with a plate.)

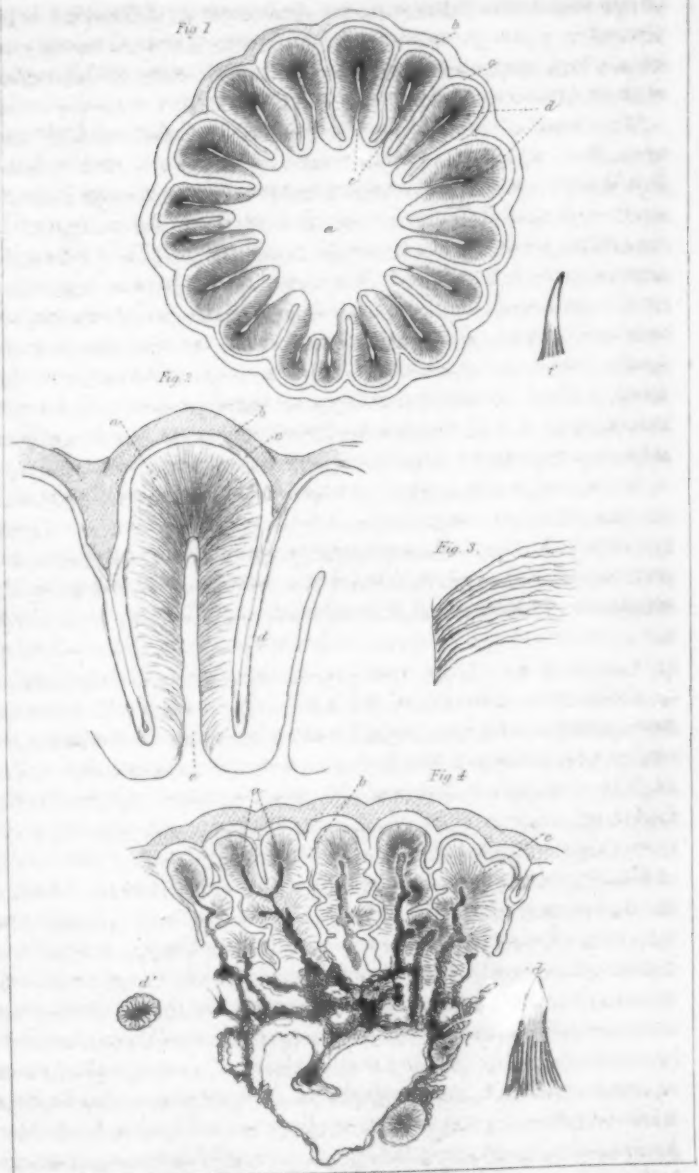
[Read before the Boston Society of Natural History, August, 1843.]

THE Lepidostei, like other Sauroid fishes, are provided with large conical teeth, intermixed with more numerous teeth of a smaller size. The larger teeth are found on the upper and lower maxillaries, and the intermaxillaries; the smaller ones are found on the same bones, and also on the vomer and palatines. On the two last they are arranged "en carde," except on the anterior portion of the vomer in the *Lepidosteus oxyurus*, where they are arranged in a linear series. The larger teeth, of which the microscopic structure is here more particularly described, are a little recurved, have a conical form, and sharp and slightly trenchant points; externally the surface is smooth near the apex, but more

or less striated or fluted near the basal portion. The base is implanted in a cavity or alveolus, with which it is anchylosed; but from which, when shed, the teeth are detached by the absorption of their substance at the points of union.

The teeth of the *Lepidosteus platyrhinus*, Raf. and *L. oxyurus*, Raf. when cut at right angles to their axes, present a surface which is subdivided into numerous segments, by lines extending from the circumference towards the centre, the whole resembling somewhat a section of a porcupine quill. Under the microscope this appearance is seen to be the result of a peculiarity of organization hitherto undescribed in the class of fishes, and to which there is an approximation in the teeth of the Ichthyosaurus, and a still closer resemblance in those of the Labyrinthodonts. With regard to the teeth of the *Lepidosteus ferox*, Raf. I am unable to give any information, since as yet I have not been able to submit them to microscopic examination.

In the *Lepidosteus oxyurus*, where the teeth are the least complicated, (though constructed upon the same plan as in the L. platyrhinus,) the basal portion is fluted, while that near the apex is perfectly smooth, which differences correspond to others in the substance of the tooth still more striking. (See fig. 1, *f*.) Externally the tooth is covered with a layer of investing substance or "cæmentum," (figs. 1 and 2, *b*,) which follows the outline of the more internal fluted portions; and within this is a second more transparent layer, (figs. 1 and 2, *c*,) which at the space between two adjoining convolutions extends in a straight or but slightly curved line towards the centre, (fig. 2, *d*,) and being folded on itself returns again towards the circumference; and this is repeated between all the different segments. The length of the involved portion is equal to a little more than one third of the diameter of the tooth. The pulp cavity, which in the mature tooth is quite small, is characterized by the existence of radiations from its circumference towards the exterior of the tooth, terminating at equal distances from its centre, and occupying the middle of the spaces between the involutions of the cæmentum, (figs. 1 and 2, *e*.) The space comprised between the involutions of cæmentum and the radiations from the pulp cavity, is filled with "dentine," which is characterized by the existence of calcigerous tubes, of which there are highly magnified representations in fig. 2, *a*, and 3; these last in nearly all cases radiate from the



J. Wyman del.

TEETH OF LEPIDOSTEI

prolongations of the pulp cavity, but very few of them coming from that cavity itself.

In the apical portion, which is perfectly smooth externally, the internal structure is much more simple than that of the basal portion which has just been described, there existing no involutions of cæmentum, and no radiations of the pulp cavity. The calcigerous tubes in the apex radiate at once from the central cavity, extending towards the circumference.

The teeth of the *Lepidosteus platyrhinus*, Raf. (fig. 4,) commonly known as the "duck-bill gar," are constructed upon the same general plan, though more complex in the details. The involuted cæmentum extends in straight lines towards the centre for a very short distance only, and then becomes more or less irregular in its course, sometimes being undulated, and at others changing its direction suddenly so as to form angles, (fig. 4, *b*.) The pulp cavity and its prolongations are also more or less irregular, according to the condition of the cæmentum, generally terminating in a simple dilatation, or, as is sometimes the case, bifurcating, as at fig. 4, *a*. In the central portion of the tooth exist also numerous pulp canals, which send off calcigerous tubes from their circumference, a conformation similar to that met with in the Rhizodus, among the extinct Sauroids.

Remarks.—In considering the structure of the teeth above described, no one can fail to recognize the analogy which exists between them and those of the Labyrinthodonts, described by Prof. Owen in his Odontography; and had we nothing but the teeth of the respective animals to which they belong with which to institute comparisons, they would both be referred at once, without doubt, to one and the same natural family. Prof. Owen does not appear to have been aware of the existence of the Labyrinthodontic structure in the Lepidostei, since in speaking of the teeth of this genus no reference whatever is made to it, and in describing the involuted cæmentum of the Labyrinthodonts he says, "such a disposition of the external substance may be traced at the base of the tooth in a few fishes, but is *more conspicuous* in the fang of the *Ichthyosaurus*."* If any one will make a comparison between the accompanying figure of the tooth of the

* Odontography, or a Treatise on the Comparative Anatomy of the Teeth, &c., by Richard Owen, F. R. S. &c. Vol. I, p. 201.

Lepidosteus oxyurus, and that of the tooth of the *Ichthyosaurus* given by Prof. Owen in his Odontography, Pl. 64 B, fig. 3, it will be at once seen that in the former there is a much closer resemblance to the Labyrinthodonts than in the latter; that is, in the *Lepidosteus oxyurus* the involutions of the cæmentum are much more extensive—although the Ichthyosaurus is described by Prof. O. as having teeth much more complicated in this respect than any existing animal.

In comparing the teeth of the Lepidostei with those of the Labyrinthodonts, those of the former will be found much less complex than in a greater portion of the latter; though quite as much and even more so, than in the *Labyrinthodon leptognathus*, figured in the Odontography, Pl. 63 B, fig. 1. Between the tooth of this last and that of the *Lepidosteus oxyurus*, there is no material difference except in the size of the pulp cavity; the radiations of this last and the involutions of the cæmentum having precisely the same relative position in both.

Thus we have the teeth of the Lepidostei, and some of the species of the Labyrinthodonts at least, reduced to the same type or plan of organization. In both the pulp cavity sends out its radiations, and in both the cæmentum is more or less prolonged inwards, at regular intervals subdividing the tooth into numerous sections. The calcigerous tubes in both cases are directed from the rays of the pulp cavity towards the investing cæmentum and its involutions.

The question very naturally presents itself, whether some of the fossil teeth from the Warwick sandstone in England and the Keuper in Germany may not be referred to some extinct Sauroid fish, rather than the Labyrinthodonts, according to the views of Prof. Owen. The former existence of gigantic Batrachian reptiles it is presumed will not be doubted, since it is based upon osteological evidence which it is impossible to controvert. But Prof. Owen informs us that in many instances the teeth from both the formations above mentioned, which were submitted to him for examination, were either mere fragments, or teeth detached from the jaws on which they grew. These he very naturally referred to the Labyrinthodonts, no such peculiarities of structure having been shown to exist in the other Vertebrata, excepting in a rudimentary form in the Ichthyosaurus, and the bases of the teeth of a few fishes. Since however the Lepidostei pre-

sent peculiarities of structure which vary only in degree from those of the Labyrinthodonts, the existence in a fossil tooth of the Labyrinthodontic structure alone, would seem to be an insufficient character for determining to which of the two natural families, Sauroid fishes or Labyrinthodonts, it might belong. These remarks involve questions which we have no means at present of deciding, and would therefore submit them for the consideration of those favorably situated for instituting such comparisons as are necessary for arriving at correct conclusions.

EXPLANATION OF THE PLATE.

Fig. 1, transverse section of the tooth of *Lepidosteus oxyurus*, Raf.; *a*, pulp cavity; *b* and *c*, cæmentum; *d*, dentine; *e*, radiations from the pulp cavity; *f*, tooth of natural size.

Fig. 2, highly magnified segment of preceding section; *a*, dentine; *b* and *c*, cæmentum; *d*, one of the involutions of the cæmentum; *e*, pulp cavity.

Fig. 3, calcigerous tubes highly magnified.

Fig. 4, portion of transverse section of the tooth of the *Lepidosteus platyrhinus*, Raf.; *a*, one of the radiations from the pulp cavity, bifurcated at its termination; *b*, one of the processes from the cæmentum, having an undulated instead of a straight course as in the Lepidosteus oxyurus; *c*, dentine; *d* and *d'*, tooth and transverse section of the natural size.

ART. XIV.—*On Vibrating Dams;* by ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy in Western Reserve College.

SOMETIME in the winter of 1841–2, my opinion was asked respecting a remarkable phenomenon noticed at Cuyahoga Falls, a village on the Cuyahoga River about eight miles from Hudson. The phenomenon consisted in the vibrations of the doors and windows, and other movable objects belonging to the buildings in the village. They were noticed at certain stages of the water, and at times ceased entirely. They were generally ascribed to a certain dam in the river, and various conjectures were formed as to the mode of their production. The subject was new to me, and I did not at first form any distinct idea of the phenomenon

itself or its cause. Some weeks afterwards, I visited the locality, and although the water was at too low a stage to exhibit the phenomenon in question, I succeeded in obtaining a pretty good description of the facts and formed my opinion as to its cause. I published a notice of it in the Ohio Observer which led to some discussion, and brought to light several similar cases elsewhere. As I have not succeeded in finding any notice of this subject in such books as I have had the opportunity of consulting, I have thought it desirable that the facts should be placed on record. I propose therefore to communicate such information as I have been able to collect, and shall conclude with some speculations as to the cause of the phenomena.

I. *Dam at Cuyahoga Falls, Ohio.*

This dam is a portion of an arc of a circle, the convexity of course being turned up stream. It is formed of hewn oak timbers one foot square, piled upon each other in tiers, all morticed firmly together, so as to form as it were one huge plank two feet thick; twelve and a half feet in breadth, and ninety feet in length, measured not between the banks, but between the points of support. Its curvature is described with a radius of a hundred and twenty feet; that is, the arc is about one eighth of the circumference of a circle. There is an embankment of earth upon the upper side, which until recently was left in an unfinished state. The bank did not rise to the top of the dam, and sloped off very abruptly. This dam was erected in the summer of 1840. During the winter of 1840–1 there was noticed considerable rattling of the windows of the neighboring houses; a phenomenon different from what had ever been noticed before; but during the winter of 1841–2, which was a very open and wet winter, the vibrations were more remarkable and became a matter of general complaint. The doors and windows of most of the houses in the village would shake for days together violently as with the ague, and to such a degree as seriously to disturb sleep. This phenomenon was apparently somewhat capricious. After continuing for a time, perhaps an hour or a day or longer, the vibrations would suddenly cease, and after some interruption might be as suddenly resumed. The rattling of vibrating objects would frequently cease, while the vibrations could still be *felt*. A window, when *apparently* at rest, if put in motion by the hand, would continue to rattle. The

buildings themselves (stone as well as wood) would vibrate with the doors and windows. This might be felt and also seen; as for example, a slender branch of a grape vine trained up against the side of a stone building, was seen to vibrate in exact time with the doors and windows.

The vibrations ceased entirely when water ceased to pour over the dam; they were also inconsiderable when the depth of water was eighteen or twenty four inches. A depth of five or six inches produced the greatest effect. The number of vibrations per second was thought to be about constant, but no accurate experiments on this point were ever made. From the best estimate I could obtain, they amounted to twelve or fifteen per second. A heavy log resting against the dam materially impaired the effect. The vibrations were seldom noticed in summer, as the water rarely run six inches over the dam; I however formed a plan during the summer of 1842, for a series of experiments during the ensuing winter and spring, to determine particularly the number of vibrations per second. I had several methods in mind to be tried if others should fail, but the one with which I expected most success was with a monochord; being simply a cat-gut, one end of which passed over a pulley and was stretched by a variable weight. The number of vibrations was too small for a musical sound, but by holding a small slip of paper near the string when vibrated, a succession of rattles is produced, which I hoped might be tuned to unison with the rattling of the windows. The vibrations of the string could be easily determined by the principles of acoustics. I was however never allowed the opportunity of testing my methods. In my first communication to the Ohio Observer, I had stated as a test of my theory, "if this dam were filled up to the top with earth it would probably cease vibrating." To my great regret the experiment was immediately tried. During the season of 1842 a large amount of rock, estimated at two hundred and fifty tons, was deposited upon the embankment. This raised the bank fully up to the level of the dam for a depth of six feet or more. Since that time the river has passed through every stage of elevation, known in ten years; from that in which you might walk with dry shoes over the entire length of the dam, to a depth of six feet on the break of the dam, which happened June 5, 1843, the greatest rise known since 1832. The result is that *the vibrations have entirely ceased.*

II. *Dam in East Windsor, Conn.*

The following information is derived from a letter from Mr. M. W. Osborn, dated Florence, Erie Co. Ohio; and another from Mr. N. S. Osborn, dated Scantic, Conn. This dam is on the Scantic River, in the township of East Windsor. It was formerly eighty feet in length; but is now one hundred, and is perfectly straight. It is based on a sandstone rock, raised from six to seven feet from the rock, and about five feet from the surface of the water below the dam. The dam is a flat one, the rafters being raised not more than twenty five degrees. It was built by raising two tiers of logs, one in front and the second much lower, some ten feet back of the front one. These are bound together, and secured by ties running from one to the other. On these tiers of logs each rafter rests, with its foot on the solid rock at the bottom of the dam. The base is covered with gravel about three fifths of the distance towards the top. The vibrations are most remarkable when the sheet of water is about four or five inches deep. A gentle breeze up the stream is said to be most favorable to the vibrations, but a high wind, disturbing the falling sheet of water, in a measure destroys the effect. An unbroken sheet the whole length of the dam is necessary to get the greatest effect, and an unbroken sheet to a considerable extent to get any effect. Any article, whether light or heavy, resting on the edge or top of the dam, thereby separating the sheet of water, impairs the effect, and hence arises the practice of nailing strips of board every twelve or twenty feet to destroy the vibrations. This is an infallible remedy. Mr. Osborn has resorted to it yearly for fifty years. It has been his uniform custom to cause strips of board from six to twelve inches in width to be nailed with their ends projecting beyond the dam sufficiently far to divide the sheet of water. The width of the boards must be taken into account. It is found from experience that the narrowest ones must be placed nearest together, while the wider ones will bear to be separated a greater distance. These pieces usually become torn off during the winter and spring freshets, but the vibrations are not felt at all while the water is high, or while the sheet that pours over the dam much exceeds four or five inches in depth. A sheet of water of much more than this depth falls with a smooth unbroken surface, and without vibratory motion.

Some effect is felt when the depth of water is less than four or five inches. When the water commences pouring over the dam in a thin sheet, the tremulous motion is perceived in the falling water. This motion increases as the water descends, and a very thin sheet is soon broken and falls irregularly. As the depth of water increases, it is less and less scattered in its fall, until at length it forms an unbroken sheet. It is not until this takes place that much effect is produced on the surrounding atmosphere. The strength of the vibrations increases with the depth of water up to four or five inches; beyond this, the vibrations diminish, until at length they cease to be felt in the atmosphere, and cannot be perceived in the sheet of falling water. The tremulous motion of the water, which is greatest at the bottom of the sheet, is not confined to the falling water, but may be seen for a short distance back of the edge of the dam.

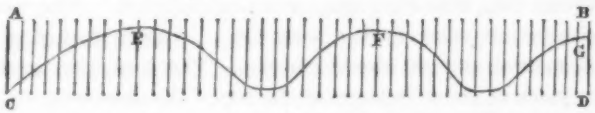
The vibrations are not at all times very perceptible to the ear. They give a fluttering sensation, like that produced by a partridge while "drumming." The number of vibrations is about five per second. A window will commence vibrating, and increase in force for five, ten or fifteen minutes, or perhaps longer, when it will gradually cease, and sometimes remain at rest for a short time. Owing, as is supposed, to the unequal thickness of the sheet of water, caused by the settling of some portions of the dam, and the breaking of some planks, the vibrations have not been experienced for the last two years, until since some recent repairs. They occur now only occasionally, and the effect is but slight in comparison with what has formerly been witnessed. The same cause has prevented them before, within the memory of Mr. Osborn. The dam has constantly been built on the same plan for fifty years or more. The most powerful vibrations ever witnessed occurred when about twenty feet of one end of the dam was about two inches higher than the rest of it. They apparently began at that end of the dam, from which they extended the whole length, when the rest of the sheet was too thick to vibrate of itself. At that time they would continue to grow more powerful until the motion was communicated to the water in the dam near to the edge, when it would cease for a few minutes. The vibrations have been known to affect windows in a house nearly one fourth of a mile distant in a northwest direction, and another a little more than one fourth of a mile in a southeast direc-

tion, the foundations of both of these buildings resting on sand. It is not recollected that any vibrations have ever been noticed in the windows on that side of Mr. Osborn's house which is turned directly from the dam.

III. *Dam at Springfield, Mass.*

The following is the substance of a letter from Mr. Amasa Holcomb, well known as the manufacturer of Herschelian telescopes. The dam crosses Westfield River about two miles west of Springfield. It is four hundred and fifty feet long and seventeen feet high, and stands on solid rock the whole length. It runs nearly north and south, and is perfectly straight and level on the top. The water is taken from the dam in a canal about eighty rods to a paper mill; and where the dam joins the canal there is a strong breastwork of stone laid without mortar. Where the dam joins the bank at the south end, there is also a breastwork of stone, with an apron of plank for a few feet, on which the sheet of water falls and turns it to the north. All the rest of the sheet falls into water of six feet or more in depth, it having torn away the rock below the dam. The dam is of wood, framed. A row of posts about three feet apart are framed into the highest plate of the dam its whole length. The upper side of the dam has a slope apparently of 40° or 50°; but on the lower side, the water falls without obstruction the whole height of the dam. The dam is built in the best manner, and was very expensive. The rafters are covered entirely with plank carefully jointed, and tight without gravel except at the bottom. There may be some gravel washed on, but there is none to be seen. The water is now (Aug. 10, 1842) running over the dam to the depth of about eight inches. The most favorable time for vibrations is when the water is a little lower, but I am told that it requires nearly the present amount to produce them. I will now describe the vibrations as intelligibly as I can. There is no wind and the surface of the pond is very smooth, perfectly so apparently, but the sheet of water is in waves, and the inequalities appear to commence immediately after the water leaves the dam. Standing on the stone breastwork at the north end of the dam, and looking south in the range of the dam, the top of it appears like a long stick of timber under water with the ends fast, but vibrating horizontally four or five inches in the middle. This

apparent vibration is no doubt caused by the shape of the surface of the water. Standing on this breastwork, my clothes shook and kept exact time with the vibrations. Standing on the canal bank of earth a few rods below, the effect on my clothes was perceptible, but much less. I was told that the vibrations were about two per second, but I found them very different. Others had not accurately observed the time, or the time is variable. There are three distinct vibrations that appear to be equidistant in time, and then a space nearly equal to two, and this in constant succession as observed from my station at the north end of the dam. The three vibrations with the space, occupied a second as near as I could measure. But then there appeared to be a finer set of vibrations between these, but how many is past human skill to ascertain with precision. But there is another very curious phenomenon that I observed. Standing twenty or thirty rods below (east of) the dam, where you look at the sheet of water nearly at a right angle, it seems to flash like lightning in dry clouds, or northern lights when there is a brilliant display of electrical flashes. This is evidently owing to the sheet of water partially breaking, and consequently looking white, while some part at the same time remained unbroken. I cannot describe this without a figure.



A B represents the top of the sheet of water; C D the bottom; the curved line above E, a kind of arch; that above F, one somewhat shorter, and at G, a part of an arch. Standing down the river and looking at the sheet, that part at G turns white or partially breaks, and the change begins at the bottom and runs up nearly to the top of the sheet. Then the part at F changes in the same manner, running rapidly from the bottom to the top, and then the part at E in the same manner. Then commencing again at G, the same order is observed without the least apparent variation. There is some change observed in that part above the curved line, but small, especially in that part near the middle of the dam, or between E and F. The changes at G, F, and E, correspond in time precisely with the vibrations. I mean by this, that the water breaks or changes white once in a second at

G, and the same at each of the other places, making three distinct vibrations in a little more than half a second, and then a longer space as before observed. The doors and windows shake in Springfield, two and a half miles from the dam.

IV, V. *Two dams in Northampton, Mass.*

The following information was furnished by Professor Hitchcock. The vibrations and jarring have occurred at two dams in Northampton, about a hundred rods apart on the same stream, running along the south side of the village. Both of them are built of wood; that is, of timber with planks pinned to them, and the top is straight. The upper one is two hundred feet long, and twelve feet high; the lower one about the same length, and six or seven feet high. Both of them rest on solid strata of red sandstone, which have an easterly dip of 15° or 20°. The vibrations occur only at a particular height of the water. If the sheet is not continuous they do not take place; nor if it be quite thick at high water. As the water was not at the proper pitch either time when I visited the spot, I have not witnessed the effects.

I was assured that the jarring of houses was sensible through the whole village, which must be over a distance of half a mile. Some of the near neighbors found the effect so annoying from the lower fall, that they threatened prosecution. The proprietors knew not what to do; when happily a log floated against the dam and stopped the vibrations. The hint was improved, and a piece of plank fastened to the top of the dam, so as to break the sheet, and since that time they have not been noticed. They are still common at the upper dam, and I was told by an intelligent man who lived upon the bank, that the vibrations could be distinctly seen, not only on the curve of the descending sheet, but extending as waves a considerable way up stream. He informed me also that he first noticed these vibrations in a mill-dam of a similar kind, built upon a rock in Brattleborough, Vt. Your inquiries have also quickened my own memory, so that I distinctly recollect having frequently noticed a singular vibration in the descending sheet of water over a dam, so that I can hardly doubt but it is common. I regret that my time does not permit me to hunt up other facts of a similar character.

VI. *Dam at Gardiner, Maine.*

The following is a letter from Mr. R. H. Gardiner. The Cobbossee Contee river, a fine mill stream, empties into the Kennebeck at this village. In the last mile of its course it falls a hundred and twenty seven feet. There are now six dams across the stream, all of them built of stone, and make a fall of from twelve to twenty two feet each. This however is not the height that the water falls perpendicularly, which varies from eight to sixteen feet. All the dams are of the same construction, that is, two walls of split stone are laid and filled in between with small stones and coarse gravel, and covered on the top with flat stones seven or eight feet long, making that the width of the dam at the top, which varies at the bottom from ten to fourteen feet according to the height. The upper side is filled with earth and gravel to within two feet of the top. The wall on the lower side is nearly perpendicular, and the water falls from seven to fifteen feet on a wooden apron of timber. These dams have been erecting during the last twenty years; but what is remarkable, the vibrations were never observed until last year, although the oldest dam, and the one which has the highest perpendicular fall, is in the midst of the village. In all these dams, the portion over which the water falls is a perfectly straight line, and varies from eighty to a hundred and sixty feet in length, and the water where it runs over the dam in freshets is from three to six feet deep.

We have had no freshet this year and no vibrations. I regretted that I did not give more attention to them last year; but living more than a mile from the village, and being particularly occupied at the time, I omitted the opportunity. The vibrations were seldom heard in the day time, but regularly in the night.

The only circumstances that I am aware of that were peculiar to the last season were, 1st, the dam immediately above the lowest was built the summer preceding. The roll of this dam, over which the water falls on the apron, is the shortest in the river. Of course the water falls here with more violence. 2nd. The freshet occurred when the frost was coming out of the ground, and the earth was full of water. I could not learn that the vibrations were felt when there was much wind. The strongest vibrations were felt not close to the dams, but in some buildings

(and one of them a brick hotel) erected on boggy ground near the river, but thirty rods below the lower dam, and ninety below the one most recently built. I intended to have made careful observations this season had the opportunity been afforded.

VII. *Dam at Hartford, Conn.*

The following is from Mr. J. P. Brace. About nine years ago, I lived near the mill-dam called Imlay's dam, and was much annoyed by the rattling of the windows of my house, when any quantity of water was passing over the dam. It was some time before I ascertained the cause, but after some investigation was fully satisfied it was produced by the passage of the water. The reason of my hesitation at first was the variation in the noise when the state of the dam was the same; but I soon discovered that the direction of the wind would account for this variation. The noise was produced by a rapid vibratory motion of the window sash, if slightly loose. I have stood by the dam and perceived that the impulse of the falling water gave the same motion to the contiguous air, and that the noise of the waterfall was not continuous. I never ascertained how much water was necessary to produce the effect, but I have the impression left upon my mind, that in a great freshet the vibratory motion ceased. Those who lived nearer the dam than I did were often very much annoyed. Within a few years, the dam has been taken down and built anew from its foundations. Whether the same effect now takes place, I cannot inform you.

Besides the seven cases here described, and an eighth alluded to at Brattleborough, I have heard of a ninth at Putnam in Ohio, and have also received vague intimations of several others.

Remarks.

In all these cases it is sufficiently obvious that the running water is the prime cause of the vibrations, for the vibrations invariably cease when the water ceases running. But how is the effect produced? By friction upon the dam? by collision with the air in its fall? by impulse upon the rock beneath? or in some other way?

1. *The dam itself vibrates.*—The experiment tried at Cuyahoga Falls admits I apprehend of no other explanation. Last year when the dam was comparatively free, the rattling of windows was

very annoying. But after the dam was loaded with two hundred and fifty additional tons of stone, all other circumstances remaining the same, the vibrations ceased. Moreover, direct evidence of the vibration of the dam is afforded at Scantic, (p. 367,) and at Northampton, (p. 370,) by the tremulous motion of the water extending a short distance back of the edge of the dam. The fact mentioned at Springfield, (p. 368,) of the apparent vibrations of the dam, is probably to be explained in the same way. Although refraction through the undulating surface of the water would give an apparent motion to the timbers of the dam, yet this undulating surface itself indicates real vibrations in the dam.

2. *These vibrations are excited in the dam by the friction of the running water.*—Vibrations of the dam must be communicated to surrounding objects, the air, water, earth, etc. In which of these are the vibrations first excited? Does the air communicate vibrations to the dam, or the dam to the air? This question is answered by the experiment at Cuyahoga Falls. When the dam is so loaded that it cannot vibrate, the phenomenon ceases entirely. Hence it is clear that the dam is the original vibrating body; and those who have observed the vibrations excited in elastic rods and plates by a bow, will probably have little hesitation in admitting that the running water performs the office of a bow. We may easily estimate the velocity requisite to produce the greatest effect. The depth of water at the time of the greatest vibrations is estimated at five or six inches for Cuyahoga Falls; four or five inches at Scantic; a little less than eight inches at Springfield; and at Gardiner, for a stone dam, about six feet. That is, a wooden dam requires a velocity of five or six feet per second, and a stone dam about twenty feet. The ordinary velocity of the bow upon a bass viol is perhaps one foot per second.

3. *The time of a vibration may be computed when we know the dimensions of the dam.*—From some experiments made with the largest beams I have been able to command, it is inferred that a single beam of white oak two feet thick and ninety feet long, vibrating as a whole, would make 1.5 single vibrations per second. Vibrating in two segments, it would make 6.0 vibrations, and in three segments 13.8 vibrations per second. The time of vibration is independent of the width when the beam is free; but if one edge of a long plank be confined, its number of vibrations is increased, while by loading it the number is diminished. I am un-

able to compute these effects rigorously for the Cuyahoga Falls dam, but presume that they nearly balance each other. The number of vibrations thus computed, on the supposition of three vibrating segments, corresponds very closely with the estimate on p. 365; it is hence inferred that this was the common, perhaps the only mode of vibration of this dam. I much regret that I had not the opportunity of measuring accurately the number of vibrations, and of obtaining some direct evidence of the number of nodes. At Springfield it appears pretty clear that there are two and a half vibrating segments, the half segment at the north end vibrating as if that extremity were entirely free. With a rod of uniform size and elasticity the first two segments would be of one hundred and eighty feet each, and the remaining half segment ninety feet. In a dam, these conditions of perfectly uniform thickness and elasticity could not be expected, and the segments may be considerably unequal. A prismatic beam of oak one hundred and eighty feet long and twenty two inches thick, would make one vibration per second. It is remarkable that the vibrations of the three segments appear not to be synchronous, but succeed each other at intervals of about a quarter of a second. If this observation was made while standing at the north end of the dam, a part of the retardation might be ascribed to the velocity of sound, which travels one hundred and eighty feet in about the sixth part of a second. It would seem that the intervals should be different when observed from one end of the dam, and from a station thirty rods below, though no mention is made of any such difference. This point deserves further examination.

The dam at Scantic is one hundred feet long, and makes five vibrations per second. It may hence be conjectured to vibrate in two segments. No direct evidence of this fact is furnished by the preceding statement. It is not improbable however that peculiarities may be detected in the falling sheet, similar to those at Springfield, which would indicate the position of the nodes.

4. *Why is the effect of the vibrations impaired by dividing the sheet of water?*—The facts stated on pp. 366, 370, may seem inconsistent with the idea that the dam is itself the original vibrating body. These facts do indeed indicate that the descending sheet of water has an important office to perform. This office I conceive to be that of a membrane vibrating in unison with the dam and reinforcing the sound. Behind this sheet, is a confined

body of air. The dam vibrating before this air is like a tuning fork before the open mouth of a tube or cubical box, the opposite end of which is closed by a stretched membrane. The box does not originate the vibrations, but reinforces them, so that effects are produced upon distant objects to which the dam itself would be entirely incompetent. This effect is impaired by every division of the sheet of water. It is a rent in the membrane. It is not to be supposed that the vibrations of this confined column of air correspond to the fundamental note of a tube of the same length. A segment of the dam at Springfield makes but one vibration per second, which would require an open tube eleven hundred feet in length to vibrate in unison with it.

5. *Are the vibrations transmitted to distant objects by the earth or atmosphere?*—Air is the more common vehicle of sound, yet as most of the dams here mentioned rest on solid rock, which is a good conductor, it has been supposed that this might be the principal conductor. There are some facts which seem to indicate that these vibrations are transmitted chiefly by the atmosphere. (1.) The two buildings mentioned p. 367, at Scantic, as being near the limit of this influence, both rest on a bed of sand, which is a very poor conductor of musical vibrations. (2.) The vibrations are chiefly heard on the side of buildings next the dam. This is asserted of Scantic, p. 368, and has also been asserted of Cuyahoga Falls. (3.) It is probable that the vibrations are chiefly confined to the upper edge of the dam, where the friction is applied and where the dam is more free. (4.) The facts mentioned at Springfield, pp. 368, 369, show that the stone breastwork vibrates, but the canal bank of earth a few rods below hardly at all. This shows how poor a conductor is sand, and renders it improbable that it should be the vehicle for transmitting the vibrations to the buildings mentioned p. 367. (5.) The fact mentioned at Gardiner, p. 371, respecting the place where the strongest vibrations were felt, may seem inconsistent with my position; but it is presumed that this statement was derived not from Mr. Gardiner's own observation but from the testimony of others, and it is possible that a general conclusion may have been drawn too hastily.

6. *Why is not the rattling of windows continuous, instead of being subject to frequent interruption?*—I do not understand that the dam ceases to vibrate whenever the windows cease to rattle.

Thus it is mentioned, p. 364, that a window when apparently at rest if put in motion by the hand, will continue to rattle; showing that there was a power previously acting upon the window, but not quite sufficient to overcome all the resistances; yet with a little foreign assistance at starting, the vibrations are maintained. The seemingly capricious motions of the window indicate to me only slight changes of *intensity* in the moving force, or in the amount of the resistances. Slight changes in the moving force may arise from a change in the depth of water upon the dam, due to the drain from the mills dependent upon it. Hence when the water is low, the phenomenon would more frequently happen by night than by day. Or it may be due to a real increase of water in the river, owing to showers at a distance. A change in the amount of the resistances may be due to the moisture of the atmosphere, temperature of the room, etc. Or finally, a slight change in the direction or force of the wind may occasion an appreciable difference in the strength of the transmitted vibrations; and common impression would seem to ascribe nearly the whole effect to this circumstance.

Hints to observers.

As this phenomenon is not known to have been particularly studied hitherto, and as it seems intimately connected with an important branch of science, it is hoped that it may receive some attention. I therefore propose to direct observers to some points which seem to merit particular examination.

1. The number of vibrations per second. This should be determined with the utmost accuracy, and frequently verified to see if the number is invariable. When the number does not much exceed four or five vibrations per second, they may be directly counted. With a seconds watch in your hand, count the number for one minute. Repeat the process several times, and take the mean result. Do the same every day for a long period, and preserve the separate results. See if the discordances exceed the unavoidable errors of observation. If they do not, it may be presumed the time is constant. When the number of vibrations amounts to ten or fifteen per second, some artifice must be resorted to. Compare the rattle of the windows with that produced by some known movement; e. g. a wheel moving with a known velocity, and having projecting teeth which strike upon some

light object, as a piece of paper held in the hand. The method described on p. 365 might perhaps be available.

2. A minute description of the dam is important. Its precise length, breadth, and height; material; form of construction; straight or curved; amount of embankment on the upper side, perpendicular or not on the lower side.

3. Peculiarities in the descending sheet of water, which indicate the vibrating segments. It seems improbable that a long dam should vibrate in one segment. The depth of water on the dam should be frequently measured, both during the occurrence and cessation of the phenomenon. It should be preserved in a register, stating for the same dates whether the vibrations were perceived and to what degree.

4. The direction and force of the wind should be noted whenever the vibrations are suddenly interrupted or resumed. It is also important to know what is the greatest distance at which the vibrations are ever felt? Are they perceived on all sides of the building; and upon what foundation does the building rest, rock, sand, clay or gravel.

5. It is desirable to get direct evidence of the vibrations of the dam. If several rods of an inch diameter were inserted vertically into the top of the dam, it is not improbable that by taking careful range with fixed objects the vibrations might be sensible. This method might detect not only the existence of vibrations, but the place of the nodes. In general, we might expect the middle of a vibrating segment where the current is swiftest, which is usually towards the middle of the dam. If any one should try this experiment expecting to see an oscillation of the rods through several inches, he would probably be disappointed. I should not however despair of being able to render the motion sensible. The rods should obviously be sufficiently rigid not to have a vibratory motion of their own independent of the dam.

In the speculations in which I have here indulged, I have been guided entirely by my own reflections, except so far as the general principles of musical vibrations are concerned. It is not improbable therefore that this article may contain various statements which I shall hereafter see occasion to correct. If however it should be the means of exciting the curiosity of scientific men, my main object will be accomplished.

ART. XV.—*Reply of* J. P. COUTHOUY, *to the accusations of J. D. Dana, Geologist to the Exploring Expedition, contained on pp.* 130 *and* 145 *of this Volume.*

Messrs. Editors—I have only this day been made aware, that in the July number of your Journal, p. 130, are the following remarks, forming part of an article "on the temperature limiting the distribution of corals, by J. D. Dana, geologist to the U. S. Exploring Expedition." "I have before stated to the Association, that the temperature limiting the distribution of corals in the ocean is not far from 66° Fahr. On ascertaining the influence of temperature on the growth of corals, I was at once enabled to explain the singular fact, that no coral occurs at the Gallapagos although under the equator, while growing reefs have formed the Bermudas in latitude 33°, four or five degrees beyond the usual coral limits. In justice to myself I may state here, that this explanation, which was published some two years since by another, was originally derived from my manuscripts, which were laid open most confidingly for his perusal, while at the Sandwich Islands in 1840. The anomalies which the Gallapagos and Bermudas seemed to present, were dwelt upon at some length in the manuscript, and attributed in the *latter* case to the influence of the warm waters of the Gulf Stream; in the *former* to the southern current up the South American coast, whose cold waters reduce the ocean temperature about the Gallapagos to 60° F. during some seasons, although twenty degrees to the west, the waters stand at 84° F." To this passage, is appended the ensuing editorial note of explanation. "The publication here alluded to we understand refers to an article by Mr. J. P. Couthouy, which appeared last year in the Boston Journal of Natural History."

I am at a loss for words that shall express the mingled feelings of astonishment, indignation and sorrow, excited in me by a perusal of the above most serious accusation, against which I claim the privilege of defending myself through your pages. In doing this I shall endeavor to be as calm and dispassionate as the case will permit. From the wording of the passage above cited, I am somewhat in doubt whether the idea intended to be conveyed by Mr. Dana, is that I am indebted to him for my views upon

temperature as connected with the growth of corals, taken as a whole, or merely for the application of those views to the anomalies presented by the Gallapagos and Bermudas. To avoid any possibility of evasion however, I shall consider his charge as embracing both these points, since in another place he certainly affirms the former of them, as will be shown before I conclude.

The broad meaning of the charge is, that I am guilty of THEFT, literary larceny; of a treacherous and most dishonorable ABUSE OF CONFIDENCE, in having appropriated the ideas developed by Mr. Dana in his MSS. "confidingly laid open for my perusal." The imputations it contains are so gross, that if substantiated, they would richly warrant my ignominious expulsion, not only from the Association before which they were made, but from every other scientific body of which I have the honor to be a member. It stands Mr. Dana in need to be very sure that he can make good his assertion, since if he fails to do so, he must appear before the public in no enviable position, as guilty of having cast a foul blot upon the escutcheon of another on insufficient grounds. It must be borne in mind, that in the distribution of the various departments of natural history among the naturalists attached to the expedition, the corals were specially assigned to me. Their habits, growth, distribution and all else connected with their history, were consequently the objects of my particular attention. Traversing the same ground with Mr. Dana, possessed of equal facilities for observing the phenomena presented by corals, with the same facts presented to my notice, (and I believe all my associates, not excepting Mr. D. himself, will do me the justice to acknowledge that I neglected no occasion for investigating either,) it must I think be admitted that something more than Mr. D.'s unsupported assertion is requisite, to prove that I could only arrive at similar conclusions with himself, by meanly purloining his MS. statements. Until it could be made manifest that Mr. D. enjoyed an exclusive monopoly of the ability to deduce the views under discussion, from a study of phenomena simultaneously observed by both; I might content myself with claiming that my simple denial should be taken as an equivalent for his bare assertion, and casting (as is my undoubted right) the *onus probandi* upon his shoulders, challenge him to produce the proof of his charge. It will not suffice for him to show that the views referred to, were contained in his MSS., and

that these latter were perused by or read to me at Oahu in 1840. He must prove beyond a question, that I then and there, from that source and no other, derived as he affirms, the views set forth by me in the article to which your note alludes. I have no intention however of resting satisfied with this alone. In answer then, to the accusation of Mr. Dana, I solemnly declare on my faith and honor as a man, that it is utterly and unqualifiedly destitute of the slightest foundation in truth, with the exception of the mere fact of his having laid open his MSS. for my perusal, in relation to which I cannot speak positively. At the next meeting of the Association, and in the presence, I trust, of every one who heard the remarks of Mr. Dana, this denial shall be substantiated by the most unequivocal testimony, personally should I live, and should I not, the proof shall be entrusted to others who will vindicate my memory. I will simply state at present, that so far from my having derived the opinions in question, as Mr. D. alleges, from his MSS. at the Sandwich Islands in 1840, they had at that period been several months in the possession of my friends in the United States, having been communicated from Sydney, New South Wales, in December, 1839, in substantially the same form as to facts, so far as the influence of temperature on corals is concerned, as that of their publication in January, 1842. At the time this was done, I was confined to my room by severe illness, the result of exposure, from which the physicians had pronounced recovery more than doubtful. The squadron was about sailing on a cruise whence it might never return. In the event of an unfavorable termination to either, my manuscripts might be lost, and with them whatever of new or important they contained, either of facts or suggestions. To guard against this contingency, I transmitted, by sure hand, to some friends in Boston, duplicate minutes of the most important of my observations from the time of our leaving the United States, to our arrival at Upolu in the Samoan group. These minutes were accompanied by a rigid injunction to allow no portion of them to be made public or to be perused, excepting by a few intimate friends of those to whom they were addressed, which injunction I may here add was faithfully observed. Events have proved that I acted wisely in adopting this course, since when at the trial by court martial of Lieut. Wilkes, a year ago, my journals and notes (deposited with him on my separation from the squadron at Oahu in 1840)

were called for in evidence, they could no where be discovered among the other archives of the expedition, deposited at the Navy Department, and Mr. Wilkes professed entire ignorance of their fate. Neither am I aware that up to the present hour, they have been found or even sought for. But for the documents referred to, I might therefore, at this moment, stand comparatively powerless to repel the accusations of Mr. Dana.

These documents are not at present in my possession, but I pledge myself to obtain and produce them in evidence at the next meeting of the American Association of Geologists and Naturalists, before whom it appears the charge of Mr. D. was first made.

The publication of my article on coral formations in the Boston Journal of Natural History, for January, 1842, was first induced by certain statements of Mr. Lyell, in one of his lectures before the Lowell Institute, which caused considerable misapprehension as to the features of some of the Polynesian islands visited by me.

Looking upon the suggestions which had presented themselves to my mind, upon the influence of temperature on the growth of corals, as of some importance, and having learned from Mr. Lyell that Mr. Darwin was about publishing an elaborate work on the subject of their distribution, &c., I concluded, after consultation with my friends, to embrace this opportunity for a brief expression of my views, and thereby avoid being forestalled by him, in case his observations had led him to similar conclusions. Unlike Mr. Dana, I deemed it highly probable that another person, observing the same facts as myself, might draw precisely the same inferences. This was my sole motive for publication.

I will now pass to the fact of my instancing the Gallapagos and Bermudas as deviations from the general limit of coral formations. From the manner of Mr. Dana's mention of *his* remarks on these groups, one would naturally infer that they were the only anomalous cases cited by *me*, as well as by himself, and therefore to be viewed in the light of collateral evidence of my having purloined his notes. But this is far from a fair statement of the case. The former are spoken of, simply as an instance occurring in the equatorial Pacific of the same singular destitution of corals characterizing a number of the intertropical islands of the Atlantic, such as Trinidad, Martin Vas, Fernando Noronha, the Cape Verds, and Canaries, and Mr. Dana will not, I presume, include these also, among those whose anomalies "were

dwelt upon at some length in the manuscript." The simple truth is, my information that the Gallapagos, and the three first named Atlantic islands, were destitute of coral, was derived on my passage from Sydney to Tahiti, in the spring of 1840, from Dr. Brown, the surgeon of the vessel, and Capt. Rugg, the commander, the latter of whom, especially had spent many months among the Gallapagos, and though utterly unacquainted with natural history, had been struck, nevertheless, with this peculiar feature, as something curious, and different from any of the other tropical islands of the Pacific, among which he had been cruising for several years. He attributed it to the sulphurous salts with which the earth at these islands is every where impregnated, affecting the water to such a degree, that corals could not live in it. I was at first inclined to coincide with him, but reflecting that this explanation could not apply to the like absence of corals in the Atlantic islands, was led to suspect that it would be found owing to the low temperature of the ocean.

This suspicion, however, I only verified while the sheets of my article were passing through the press, by an examination of the meteorological tables in the appendix to King and Fitzroy's voyage, for a knowledge of which I was indebted to my friend, Dr. A. A. Gould. From the same work, and at the same time, were derived all the local temperatures of the Pacific, specified in my article.* On the other hand, the fact of coral reefs existing at Bermuda, so far beyond their general limits, is a fact in itself so remarkable, that the most casual observer would scarcely fail to have his attention drawn to it, in connection with this subject of temperature; and I think it will be conceded, that to have passed over them in silence would have been far more extraordinary, than that I should have remarked concerning them, "though unable to speak positively from having no data; as to the Bermudas, I have no doubt from their proximity to the Gulf Stream, that they are washed by an equally warm sea."† This is the extent of my observations upon "anomalies," which were "dwelt upon at some length in the manuscript" of Mr. D., "laid open most confidingly" for my perusal at the Sandwich Islands, and thus much, I trust, it has been shown I had other means of arriving at, without abusing the confidence of Mr. D. But I

* Vide Boston Journal Nat. Hist., Vol. IV, p. 160. † Op. cit. p. 160.

must now proceed to notice another, and very important mis-statement of Mr. Dana's. On page 145 of your last number, I find the following among the "proceedings of the American Association of Geologists and Naturalists," at Albany, in April last. "Mr. Dana alluded to a statement made by Mr. Couthouy, at the meeting of the Association at Boston, that the limiting temperature of corals was 76° Fahrenheit, and took occasion to remark, that Mr. Couthouy was indebted to him (Mr. D.) for the views there advanced by him, with regard to the temperature limiting corals; and added, that the temperature 76° Fahr. was a mistake by Mr. Couthouy for 70°, the limit fixed upon by Mr. Dana when the views were communicated by him to Mr. Couthouy."

It must be admitted that the language of this accusation is sufficiently clear and explicit, as to time, place and circumstance. There seems to have been especial care taken to prevent any possibility of misapprehension as to the precise nature of the charge, and also to fix it distinctly upon me, by the frequent iteration of my name. It is open but to the solitary objection that there is not one syllable of truth in the passage from beginning to end, so far as Mr. Dana is concerned. Both the statement alluded to, and the views he represents me as having expressed in it, are entirely the creation of Mr. D.'s singularly imaginative brain. The facts set forth in the indictment on which I am thus arraigned at the bar of public opinion, are altogether fictitious. The opinions which Mr. Dana therein alleges were derived by me from his manuscript, I have never expressed either orally or in print! Incredible as this may seem, I shall now proceed to place it beyond the shadow of a doubt.

First, then, I deny that I made any statement, advanced any views or expressed any opinions whatever to the Association, upon the subject of temperature limiting corals, or upon their growth, their distribution, or in short, upon any topic connected with corals directly or indirectly.* For the evidence of this assertion, I refer to the proceedings of the Association, as published in your Journal, and in the first volume of its Transactions.

* Perhaps I ought in so sweeping a denial, to except the remarks alluded to on page 153, Am. Jour. for July, 1842, (p. 48, Transactions of the Association,) on the evidences of successive paroxysmal elevations, presented by one of the uplifted coral islands visited by me in 1839; though these had not the slightest bearing on the question here at issue.

Omitting for the present farther comment on his assertion that the opinions here falsely quoted as mine were derived from him, I affirm in the next place, that Mr. Dana is guilty of gross and inexcusable misrepresentation of my actual views in regard to temperature limiting the growth of corals. I have never named, either directly, or otherwise, any particular standard of temperature as limiting such growth, but on the contrary, have declared that we were not yet possessed of sufficient data to establish that point, and Mr. Dana betrays that he feels the weakness of his cause, by thus ascribing to me opinions I have never entertained. I leave others to pass judgment on his motives for doing this, merely remarking, that could it be made to appear that I had named, or intended to name, as a limiting temperature, that designated by himself, it would give a coloring of probability to his charge.

Moreover, so far from specifying 76° Fahrenheit as such limit, by mistake, as he asserts, for 70° Fahrenheit, the limit assigned by him, I have in my published views expressly stated my conviction, that wherever this temperature of 76° exists, there corals will be found to flourish in their utmost profusion. In proof of this, I adduce the following extracts from my article on coral formations in the Pacific. Speaking of a reef near Tutuila, one of the Samoan group, on which were thirteen fathoms water, I remark, "This ledge, distant about two and a half miles from the coast, which was very steep, *was profusely covered with coral.* The *surface temperature* was here 81°, and that of the *bottom* 76° Fahrenheit. Throughout the Coral Archipelago to the eastward of Tahiti, the *surface temperature* ranges from 78° to 81°. The same may be said of that in the neighborhood of the detached islets, between Tahiti and Samoa, to the west. *Throughout this region, I observed all kinds of corals flourishing in perfection on the outer plateau of the reefs, at a depth of seven, eight, and in some cases, as that just cited, twelve or thirteen fathoms.*"* That I here intended to prove, that as throughout the Archipelago, where corals flourish in such perfection, the *surface* temperature is the same as at the reef off Tutuila; so also is the temperature of the *bottom*, i. e. 76° Fahrenheit,—is surely obvious, even without what here follows. "It is my belief that

* Boston Jour. Nat. Hist. Vol. IV, pp. 74, 75.

to a certain extent, the corals are limited in their range of growth by temperature rather than depth, and that wherever this is not below 76° Fahrenheit, *there, cæteris paribus, they will be found to flourish as in the Polynesian seas;*"* and again, "among the Paumotus, *the field of their most lavish display*, the temperature varies from 77° to 83°. At Tahiti, from 77° to 80°, and about the same at the large groups to the west of it. At the Hawaiian Islands, laying between 19° and 22° north latitude, it is sometimes as high as 81°. In our own hemisphere, among the Antilles, Bahamas, and southern coasts of Florida, I have found the temperature of the water near the shore, at different seasons, from 78° to 82°, *and in all these regions coral reefs abound.*"† I have italicised in the preceding quotations, the passages proving most clearly the falsity of Mr. Dana's representation of my opinions in regard to a limiting temperature, and I appeal with confidence to every candid and honorable mind, whether they do not completely disprove his assertion, that I named 76° Fahrenheit as the temperature limiting the growth of corals? whether, on the contrary, I have not specially designated it as the temperature suited for their utmost development?

Certainly, no unprejudiced person will attempt to deny, that there is a wide difference between affirming, that wherever the temperature is not below 76° Fahrenheit, *there corals will be found to flourish in perfection*—that where that exists, is "*the field of their most lavish display*," and stating that when it *is* below that, they will not grow at all. To deny this, would involve the utter absurdity of assuming that there were no intermediate grades of temperature, between the one suited to their most lavish growth, and that in which they become extinct. In this matter I claim only to be allowed to mean what I have said, in regard to temperature as limiting the growth of corals, and protest against its being assumed that I mean any thing more. I ask that the language of my article on coral formations be taken in its strict literal import, and I challenge Mr. Dana to point to a single pas-

* Boston Jour. Nat. Hist. Vol. IV, p. 76.

† Ibid. p. 160. Although it is perhaps unnecessary, I will here remark, that the temperatures here given are unless the contrary is expressly mentioned, those of the *surface*, as shown by a common thermometer placed in a bucket of freshly drawn water, or held in the sea by hand, while sailing or pulling along the reefs in a boat.

sage therein, which by any construction can be made to imply that I considered 76° Fahrenheit as the limiting temperature of corals. Unless he can do this, I maintain his affirmation that I named 76° by mistake for 70°, the limit named by him, to be wholly unfounded.

There can be no excuse or palliation offered for the conduct of Mr. Dana on this point. He was bound in honor, and by every principle of justice, to possess himself fully of my real opinions, before bringing against me an accusation of so serious import as that contained in the record. It matters not whence my views were derived, I had a right to claim at his hands a fair presentation of them to the public. It was especially incumbent on Mr. Dana while accusing *me* of behavior the most dishonorable, not to show any thing like an approach to unfairness *himself:* how much more then, to avoid attributing to me in support of his charges, in an imaginary statement, opinions which I have never expressed.

The records of the Association were upon the table before him at the time his remarks were offered; he had but to open them and ascertain that I had made no such statement as he alluded to. My published views were within his reach. A slight examination would have sufficed to convince him that I had never advanced those he attributed to me, and accused me of borrowing from his MSS. Between the time of his making the charge against me before the Association, and that of its publication in your Journal, nearly if not quite three months elapsed, and yet he made no attempt to correct his misrepresentations. How far these will strengthen belief in, or cast discredit upon the similar charge against me in the article first alluded to in this reply, it is not my province to determine.

A few words as to the mere fact of Mr. Dana's having shown me his MSS. at Oahu, in 1840. Although I have not the slightest recollection of the fact, I am perfectly willing to concede that it is very possible he did so.

That he exhibited his portfolio of *drawings* I distinctly remember, and how much I was struck in a cursory examination of them, with their wonderful beauty of coloring and minuteness of detail. During the few days we passed in company, we were both very much occupied by other matters, and what conversation or comparison of observations took place between us,

was of necessity very brief and hurried. My own personal relations with the commander of the Expedition were in a painfully unsettled state, and engrossed almost my entire thoughts and attention.

If I really saw Mr. Dana's notes, it will not be considered strange, that the circumstance should have escaped my memory amid the excitement and distractions of the occurrences which led to my separation from the squadron; especially when it is understood that saving my official correspondence, I am without note or memorandum of any description, relative to the Expedition, or aught that transpired at the island, subsequent to the arrival of the first of the squadron.

Had I remembered any such expression of Mr. Dana's views as he refers to, I should gladly and unhesitatingly have cited a witness so competent, in support of my own, whose priority I had abundant means of establishing. We certainly conversed freely together on all subjects so far as we had opportunity, as was natural for persons engaged in kindred pursuits, just meeting after a year of separation. If Mr. Dana, as he alleges, (and I have no objection to admit,) submitted his MSS. to my perusal, assuredly he had as free access to mine.

I may hereafter take occasion to show that he has availed himself of them in a manner that leaves him, *to say the least*, equally open with myself to the charge of having misused confidence. My first duty will be to fully vindicate myself from the accusations he has brought against me. When this shall be accomplished, it may then be Mr. D.'s turn to act upon the defensive.

I trust that I may be pardoned for here observing, that during the whole period of my connection with the Expedition, I neglected no opportunity for noting facts, and making collections in Mr. Dana's departments, both of which were always freely turned over to him, without other return being made or sought than the satisfaction it afforded me to add my contribution to the general stock. In no solitary instance did I return from an excursion without some addition to his collections. The additions thus made by me, numbered many hundreds of specimens in both Mr. D.'s departments—a large proportion of them from localities unvisited by him. I received in exchange, at the extent, some three or four dozen specimens in my departments, and the unmerited charge of having abused his confidence. At the very time he

accuses me of having done this, besides freely submitting to him all my notes on the geology of Hawaii, made during a residence of nearly six months, I placed in his hands a set of over four hundred specimens, forming a complete suite of all the formations in the group, from its northwestern to its southeastern extremity, illustrative of the facts noted, and collected along the whole course of journies over more than three hundred and fifty miles, with great care and labor, and in some instances at no slight peril to life and limb.

I have hastily drawn up this preliminary defence with feelings akin to those excited by a first perusal of the charge, not more in anger than in sorrow. I confess that after the peculiar intimacy which subsisted between us during the whole of our connection in the Expedition, after the warm expressions of regard and indebtedness on his part at the time of our separation, I was wholly unprepared for his adopting so violent a course, without a word of remonstrance or request for explanation. Had he proffered either, in lieu of assailing me unawares, while I was not present to defend myself, and leaving me to arrive at a knowledge of the fact by mere accident, I could readily have proved to him that he was under an erroneous impression, have spared much pain to one, and I cannot but hope to both of us, and prevented an act of great injustice, which he will hereafter regret no less than myself.

That he saw fit to proceed as he has done, must ever be to me a source of deep regret—but this cannot be recalled. In self defence, I am compelled to prove, that Mr. Dana is virtually a traducer, or become myself an object of scorn to all true men. I am content to abide the issue. The grounds of my defence are now before your readers. It only remains for me to assure those friends to whom the charges not herein fully disproved, have given pain, that if at the next meeting of the Association before which they were originally preferred, I fail to prove their entire groundlessness, I will consent not only to forfeit that esteem which is dearer than life, but to be branded with the full ignominy which should justly attach to conduct so unworthy as they attribute to me.

Statement of Mr. Couthouy in relation to Prof. H. D. Rogers.

Permit me before concluding, to correct an erroneous statement in the proceedings of the Association in Boston, published

in your Journal for July, 1842, which by implication conflicts with the claims of another to originality in conceiving a great theory. On page 183, (page 75 also of Transactions,) it is stated, that "Mr. Couthouy read some extracts from his journal, 'on the wave-like undulations of the earth's crust, at all periods of disturbance from the most ancient date to the present time.'" I am ignorant how this came to be so worded, but it conveys an entirely erroneous idea. The extract read by me referred *exclusively* to results produced by recent volcanic action in Hawaii, strikingly illustrative on a minor scale of those grand undulations of the earth's crust, so eloquently accounted for by Prof. H. D. Rogers, on the principle of a tremendous billowy movement of the ignifluous mass beneath, at some remote period. I read nothing referring to undulations of this description as observed by myself. My remarks were introduced at the request of Prof. R. on account of their bearing on his theory, as proving that effects similar to those described by him, were produced on a diminutive scale, by a less activity of the same agent, and also to prove the singular coincidence of expression between two observers of like phenomena, placed thousands of miles apart, occurring in his notes and my own; the same comparison of the undulations to the march of ocean waves, having been made by both in terms almost *verbatim* the same.

I request you to make this correction, that it may not be hereafter surmised from the record as it now stands, that any thing advanced by me, conflicted with the claim of Prof. Rogers to entire originality in the views then presented by him. Allow me to add, that the coincidence above referred to, is not without value for its bearings on the question raised by Mr. Dana, between whom and myself the similarity of expression is far less striking than this, which was assuredly the result of mere accident.

Respectfully, your obedient servant,

JOSEPH P. COUTHOUY,

Late Mem. Scien. Corps U. S. Expl. Exped.

341 Broadway, New York, August 28, 1843.

ART. XVI.—*Experiments made with one hundred pairs of Grove's Battery, passing through one hundred and sixty miles of insulated wire;*—in a letter from Prof. S. F. B. MORSE, to the Editors, dated New York, Sept. 4th, 1843.

Dear Sirs,—On the 8th of August having completed my preparations of one hundred and sixty miles of copper wire for the electro-magnetic telegraph which I am constructing for the government, I invited several scientific friends to witness some experiments in verification of the law of Lenz, of the action of galvanic electricity through wires of great lengths.

I put in action a cup battery of one hundred pairs, which I had constructed, based on the excellent plan of Prof. Grove, but with some modifications of my own, economizing the platinum.

The wire was reeled upon eighty reels, containing two miles upon each reel, so that any length from two to one hundred and sixty miles could be made at pleasure to constitute the circuit.

My first trial of the battery was through the entire length of one hundred and sixty miles, making of course a circuit of eighty miles, and the magnetism induced in my electro-magnet, which formed a part of the circuit, was sufficient to move with great strength my telegraphic lever. Even forty-eight cups produced action in the lever, but not so promptly or surely.

We then commenced a series of experiments upon decomposition at various distances. The battery alone (one hundred pairs) gave in the measuring guage in one minute, 5.20 inches of gas. When four miles of wire were interposed, the result was 1.20 inches—ten miles of wire, .57 inch—twenty miles, .30 inch—fifty miles, .094.

The results obtained from a battery of one hundred pairs are projected in the following curve.

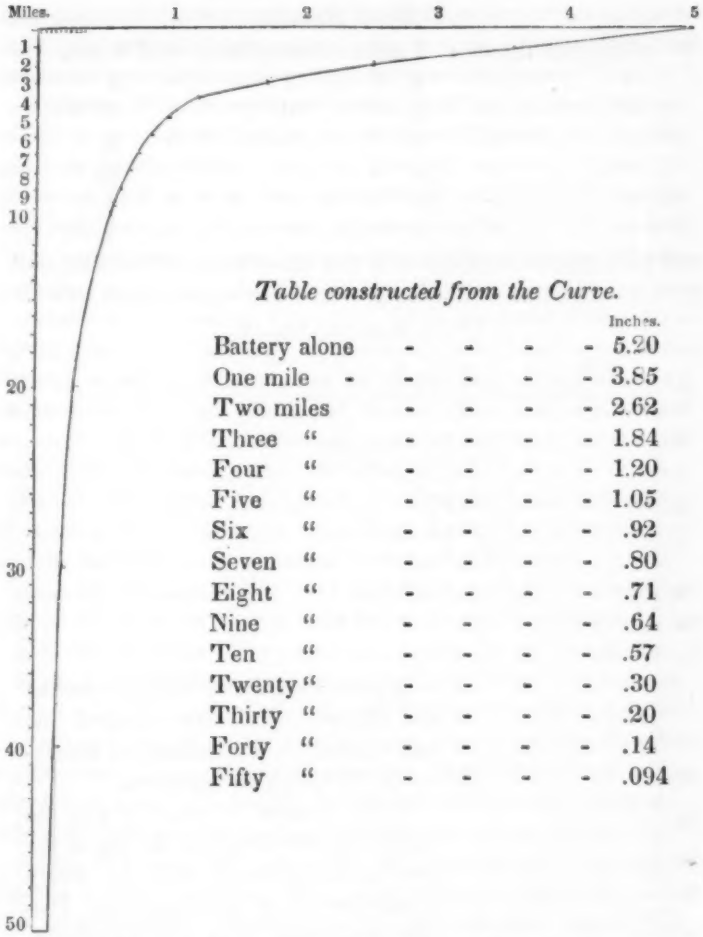


Table constructed from the Curve.

	Inches.
Battery alone	5.20
One mile	3.85
Two miles	2.62
Three "	1.84
Four "	1.20
Five "	1.05
Six "	.92
Seven "	.80
Eight "	.71
Nine "	.64
Ten "	.57
Twenty "	.30
Thirty "	.20
Forty "	.14
Fifty "	.094

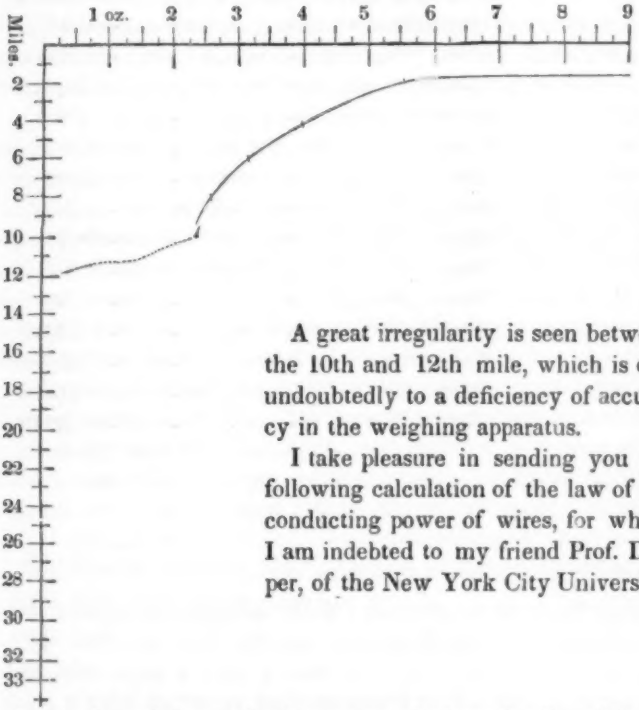
During the previous summer I made the following experiments upon a line of thirty-three miles, of number 17 copper wire, with a battery of fifty pairs. In this case, I used a small steelyard with weights, with which I was enabled to weigh with a good degree of accuracy the greater magnetic forces, but not the lesser, yet sufficiently approximating the recent results to confirm the law in question.

Table of Results.

Fifty pairs through	2	miles	attracted and raised	9	ozs.
"	"	4	"	"	"	4	"
"	"	6	"	"	"	3	"
"	"	8	"	"	"	$2\frac{1}{2}$	"
"	"	10	"	"	"	$2\frac{1}{4}$	"
"	"	12	"	"	"	$\frac{1}{8}$	"
"	"	14	"	"	"	$\frac{1}{8}$	"

and each successive addition of two miles up to thirty-three, still gave an attractive and lifting power of one-eighth of an ounce.

Curve from the Results.



A great irregularity is seen between the 10th and 12th mile, which is due undoubtedly to a deficiency of accuracy in the weighing apparatus.

I take pleasure in sending you the following calculation of the law of the conducting power of wires, for which I am indebted to my friend Prof. Draper, of the New York City University.

On the law of the conducting power of wires; by John W. Draper, M. D., &c. &c.

It has often been objected, that if the conducting power of wires for electricity was inversely as their length, and directly as as their section, the transmission of telegraphic signals through

long wires, could not be carried into effect, and even the galvanic multiplier, which consists essentially of a wire making several convolutions round a needle, could have no existence.

This last objection was first brought forward by Prof. Ritchie, of the University of London, as an absolute proof that the law referred to is incorrect. There is, however, an exceedingly simple method of proving that signals may be despatched through very long wires, and that the galvanic multiplier, so far from controverting the law in question, depends for its very existence upon it.

Assuming the truth of the law of Lenz, the *quantities* of electricity which can be urged by a constant electromotoric source through a series of wires, the lengths of which constitute an arithmetical ratio, will always be in a geometrical ratio. Now the curve whose ordinates and abscissas bear this relation to each other is the logarithmic curve whose equation is $a^y = x$.

1st. If we suppose the base of the system which the curve under discussion represents be greater than unity, the values of y taken between $x=0$ and $x=1$, must be all negative.

2nd. By taking $y=0$ we find that the curve will intersect the axis of the x's at a distance from the origin equal to unity.

3rd. By making $x=0$ we find y to be infinite and negative.

Now these are the properties of the logarithmic curve which furnish an explanation of the case in hand. Assuming that the x's represent the quantities of electricity, and the y's the lengths of the wires, we perceive at once that those parts of the curve which we have to consider lie wholly in the fourth quadrant, where the abscissas are positive and the ordinates negative.

When, therefore, the battery current passes without the intervention of any obstructing wire, its value is equal to unity.

But as successive lengths of wire are continually added, the quantities of electricity passing, undergo a diminution at first rapid and then more and more slow. And it is not until the wire becomes infinitely long that it ceases to conduct at all; for the ordinate $-y$, when $x=0$, is an asymptote to the curve.

In point of practice, therefore, when a certain limit is reached the diminution of the intensity of the forces becomes *very small*, whilst the increase in the lengths of the wire is vastly great. It is, therefore, possible to conceive a wire to be a million times as long as another, and yet the two shall transmit quantities of elec-

tricity not perceptibly different, when measured by a delicate galvanometer.

But under these circumstances if the long wire be coiled so as to act as a multiplier, its influence on the needle will be inexpressibly greater than the one so much shorter than it.

Further, from this we gather that for telegraphic despatches, with a battery of given electromotoric power, when a certain distance is reached the diminution of effect for an increased distance becomes inappreciable.

ART. XVII.—*On the Fossil Foot-prints of Birds and Impressions of Rain-drops in the Valley of the Connecticut;* by CHARLES LYELL, Esq., V. P. G. S.*

THE deposit in which these impressions, long known on account of the researches of Prof. Hitchcock, occur, is situated in a trough of hypogene rocks, about five miles broad, the strata, which consist of sandstone, shale and conglomerate, dipping uniformly to the east at angles that vary from 5° to 30°. Mr. Lyell first examined the red sandstone at Rocky Hill, three miles south of Hartford, in Connecticut, where it is associated with red shale and capped by twenty feet of greenstone. Many of the beds are rippled, and cracks in the shale are filled by the materials of the superincumbent sandy layer, showing, the author observes, a drying and shrinking of the mud while the accumulation of the strata was in progress. The next quarries he examined were at Newark, in New Jersey, about ten miles west from New York city. The excavations are extensive, and the strata dip, as is usual in New Jersey, to the northwest, or in an opposite direction to the inclination in the valley of Connecticut, a ridge of hypogene rocks intervening. The angle is about 35° near Newark. The beds exhibited ripple-marks and casts of cracks, also impressions of rain-drops on the upper surface of the fine red shales. Mr. Lyell states, that he felt some hesitation respecting the impressions first assigned to the action of rain by Mr. Cunningham of Liverpool, but he is now convinced of the justness of the inference, having observed similar markings produced on very soft mud by rain at Brooklyn, in Long Island, N. Y. On the same mud were the foot-prints of fowls, some of which had been made before the rain and some after it.

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* Communicated to the Geological Society of London, and extracted from Vol. III, No. 91, of their Proceedings. We had not seen this abstract of Mr. Lyell's paper when the article on the Ornithichnites appeared in our last number, or we should have united it with the other matter of that paper.—*Eds.*

Mr. Lyell next visited the red and green shales of Cabotville, north of Springfield in Massachusetts, where some of the best Ornithichnites have been procured, chiefly in the green shale. The dip of the beds is 20° to the east, a higher inclination, the author says, than could have belonged to a sea-beach. He observed in the same quarries ripple-marks as well as casts of cracks, and he was informed that the impressions of rain-drops have likewise been found.

In company with Prof. Hitchcock, Mr. Lyell afterwards examined a natural section near Smith's Ferry, on the right bank of the Connecticut, about eleven miles north of Springfield. The rock consists of thin-bedded sandstone with red-colored shale. Some of the flags are distinctly ripple-marked, and the dip of the layers on which the Ornithichnites are imprinted, in great abundance, varies from eleven to fifteen degrees. Many superimposed beds must have been successively trodden upon, as different sets of tracks are traced through a thickness of sandstone exceeding ten feet; and Prof. Hitchcock pointed out to the author that some of the beds exposed several yards farther down the river, and containing Ornithichnites, would, if prolonged, pass under those of the principal locality, and make the entire thickness throughout which the impressions prevail, at intervals, perhaps twenty or thirty feet. Mr. Lyell, therefore, conceives that a continued subsidence of the ground took place during the deposition of the layers on which the birds walked.

It has been suggested, but the opinion has not been adopted by Prof. Hitchcock, that the eastward slope of the beds represents that of the original beach. With a view to this question, Mr. Lyell examined the direction of the ripple-marks, and found that it agreed with the dip, or was at right angles to the supposed line of beach; but he adds, though this agreement presents a formidable objection to the suggestion above alluded to, if the ripples were produced by waves, yet it does not disprove the opinion, as the ripples do not exceed in dimensions those which are produced by sand blown over a muddy beach, and often distributed at right angles to the coast-line. Instances of this effect of the wind Mr. Lyell has remarked along the shores of Massachusetts. Nevertheless he is of opinion that the rippled layer of sandstone in question contains too much clay to have resulted from blown sand, and he is disposed to think that in most of these localities the strata have been tilted, instances of such disturbance having been pointed out to him by Prof. Hitchcock in the state of Massachusetts, and by Dr. Percival near New Haven, in Connecticut. In reference to this subject, he says, that a few miles from Smith's Ferry, a conglomerate, several hundred feet thick, containing angular and rounded fragments of trap and red sandstone, the base being sometimes a vesicular trap and trap tuff, passes

upwards into the very flags on which Ornithichnites occur; and from this he infers, that there were eruptions of trap, accompanied by upheaval and partial denudation, during the deposition of the red sandstone.

With respect to the impressions having been made by birds, Mr. Lyell states, that until he examined the whole of the evidence he entertained some scepticism, notwithstanding the luminous account given by Prof. Hitchcock. In proof of their being the foot-prints of some creature walking on mud or sand, he mentions, 1st, the fact of Prof. Hitchcock's having seen two thousand impressions, all, like those he had himself examined, indented in the upper surface of the layer, the casts in relief being always on the lower surface; and 2dly, that where there is a single line of impressions, the marks are uniform in size, and nearly uniform in distance from each other, the toes in the successive steps turning alternately right and left. Such single lines, Mr. Lyell says, indicate that the animal was a biped, and the trifid marks resemble those which a bird leaves, there being generally a deviation from a straight line in any three successive prints; and his attention having been called to indications of joints in the different toes, he afterwards clearly recognized similar markings in the recent steps of coots and other birds on the sands of the shores of Massachusetts. Prof. Hitchcock has shown, that the same impression extends through several laminæ, decreasing in distinctness in proportion as the layer recedes from that in which it is most strongly marked, or in proportion as the sediment filled up the hollows and restored the surface to a level; and Mr. Lyell states, that he has observed a great number of instances of this fact.

He also says, that he can scarcely doubt that some of the impressions on the red sandstone of Connecticut are not referable to birds, but he believes that the gigantic ones described by Prof. Hitchcock are Ornithichnites. At Smith's Ferry they are so numerous that a bed of shale many yards square is trodden into a most irregular and jagged surface, so that there is not a trace of a distinct footstep; but on withdrawing from this area to spots where the same tracks are fewer, the observer, Mr. Lyell says, is forced to admit that the effect in each case has been produced by this cause.

On examining the shores on some small islands about fifteen miles southeast from Savannah, the author was struck with the number as well as the clearness of the tracks of raccoons and opossums imprinted in the mud during the four preceding hours, or after the tide had begun to ebb. At one spot, where the raccoons had been attracted by the oysters, the impressions were as confused as when a flock of sheep has passed over a muddy road; and in consequence of a gentle breeze blowing parallel to the line of cliffs composed of quartzose sand, the tracks had in many places already become half filled with blown sand,

and in others were entirely obliterated; so that if the coast should subside, the consolidation of this sand would afford casts analogous to those of Storeton Hill in Cheshire, yet the impressions had been made and filled in a few hours.

When considering the broad question whether the fossil foot-prints were made by creatures walking on mud or sand after the ebbing of the tide, Mr. Lyell reminds his readers of the fact that in the United States, as in Saxony and Cheshire, the tracks in sandstone and shale are accompanied by littoral appearances, as ripple-marks, the casts of cracks in the clay, and often by the marks of rain.

In regard to the age of the red sandstone of the valley of the Connecticut and New Jersey, the author states he has nothing to add to what had been previously advanced, by which its position had been shown to be between the carboniferous and cretaceous series. In the neighborhood of Durham, Connecticut, he had collected in the sandstone, fishes of the genera Palæoniscus and Catopterus, but no other organic remains, except fossil wood.

In conclusion, Mr. Lyell remarks, 1st, that the Ornithichnites of Connecticut should teach extreme caution in inferring the non-existence of land animals from the absence of their remains in contemporaneous marine strata; 2dly, that when this red sandstone of Connecticut was deposited, there was land in the immediate vicinity of the places where the Ornithichnites occur; and that but for them it might naturally be inferred that the nearest land was several miles distant, namely, that of the hypogene rocks which bound the basin of the Connecticut. Now, the land that caused the sea-beach, Mr. Lyell says, must have been formed of the same sandstone which was then in the act of accumulating, in the same manner as where deltas are advancing upon the sea.

In a postscript, Mr. Lyell states, that subsequently to writing the paper, he had read the luminous report of Mr. Vanuxem* on the Ornithichnites described by Prof. Hitchcock, and though it agrees in substance with his own account in some particulars, yet that he has left his notice as it stood.

Art. XVIII.—*Bibliographical Notices.*

1. *Zoology of New York, or the New York Fauna, comprising detailed descriptions of all the Animals hitherto observed within the State of New York, with brief notices of those occasionally found near its borders, and accompanied by appropriate illustrations;* by James E. De Kay. Part I, Mammalia. pp. 146, 4to, plates.—The mammiferous

* See this Journal, Vol. xli, p. 165.

animals found within the limits of the state of New York, as well as of the other eastern and middle states, had been already, for the most part, described previous to the commencement of the survey of which the present volume forms the first of a series of reports, published under the direction and at the expense of the state government. Many of the descriptions were drawn up by foreign naturalists, and in consequence of having been frequently made from stuffed skins and the incorrect information of travelers, were oftentimes erroneous. Within a few years, our own naturalists have turned their attention to the subject, and although but comparatively few additions have been made to the number of species, yet our knowledge is based upon a much more certain foundation. In the present work, the whole subject as regards the Mammalia has been gone over anew, the descriptions re-examined or rewritten, and such information added as the author, from his extended observations and long familiarity with the subject, was enabled to do. A copious synonymy has been made out, and many interesting observations brought together on the habits and geographical distribution of the different species.

The state of New York covers a large tract of territory, extending over eight degrees of longitude, and from 40° 30′ to 45° north latitude, and having an area of about 46,000 square miles. Of Mammalia inhabiting the State and indigenous to it, there are, according to Dr. De Kay, about seventy-four species, belonging to the following orders and natural families:

Order Marsupiata.—Didelphidæ, 1 species.

Order Carnivora.—Vespertilionidæ, 5 species; Sorecidæ, 8; Ursidæ, 3; Mustelidæ, 7; Lutridæ, 1; Canidæ, 4; Felidæ, 3; Phocidæ, 2. Total, 33.

Order Rodentia.—Sciuridæ, 6; Arctomidæ, 1; Gerbillidæ, 1; Castoridæ, 2; Hystricidæ, 1; Muridæ, 11; Leporidæ, 2. Total, 24.

Order Ungulata.—Elephantidæ, (fossil,) 3; Cervidæ, 5. Total, 8.

Order Cetacea.—Balænidæ, 4; Delphinidæ, 4. Total, 8.

As regards the geographical distribution, the following instances are interesting, as showing the great extent of territory over which some of the species inhabiting the State are found. The Vespertilio Noveboracensis is found throughout the territory lying between Massachusetts and the Rocky Mountains, and between the twenty-third and forty-second parallels of north latitude. Vespertilio pruinosus, Say, is found in nearly every state of the Union, on the Columbia River, and as far north as the fifty-third degree. Scalops aquaticus, or shrew mole, extends from the Atlantic to the Pacific, and from Carolina to 50° north latitude. The Procyon lotor, or raccoon, is found throughout nearly the whole of North America, its highest range as yet known being 60° north. Me-

phitis Americana, or skunk, is found in both Americas, extending from the frozen regions of the northern, to Chili and Paraguay in the southern hemisphere. Lutra Canadensis and Braziliensis, which Dr. De Kay regards as identical species, there being no essential differences, extends over the immense tract of country lying between the Arctic seas and Brazil.

The number of extinct mammifers whose remains have as yet been discovered within the limits of the state, is very small, and even these have been detected more abundantly elsewhere. The fossil elephant, E. primigenius, has been found in one locality only. The remains of the Mastodon giganteum, Cuvier, have been detected in several localities; but since they have not as yet been detected in Massachusetts or other eastern states, excepting Connecticut, New York may be regarded as enclosing a portion of the eastern limit.

The only other fossil mammiferous remains indicated by Dr. De Kay, are those of the fossil stag, Elaphus Americanus, which have not been detected except in one or two instances. J. W.

2. *Monographies D'Echinodermes Vivans et Fossiles;* par Louis Agassiz. Neuchatel, Suisse, 1841 and '42. 4to, planches.—Our knowledge of the Echinodermata has within a few years been much increased, especially by the labors of Muller, Tiedemann and others in Germany, and by Mr. Forbes in England. The "History of British Starfishes," by the last named naturalist, although confined to such species as are met with on the coast of England, yet may be considered as the first work in which an attempt has been made to unite in a single monograph all the different orders of the Echinodermata. In the magnificent monographs now in the course of publication by M. Agassiz and his collaborators, are included all the fossil as well as existing species hitherto known, illustrated by numerous and well executed plates. No effort has been spared to render the descriptions as complete as possible, including what has been for the most part overlooked by previous naturalists, viz. the internal organization. The importance of such a plan must be at once apparent, since it should be no less the province of the zoologist to ascertain what animals existed on the earth's surface during the early history of the world, than those which are found at the present day, and it is evident that the study of organization will be hereafter inseparable from the correct methods of zoological research.

The first monograph contains descriptions of *Salenies*, by M. Agassiz; the second, of the *Scutellæ*, by M. Agassiz; the third, *Galerites* and *Dysasters*, by Desor; and the fourth, the anatomy of the *Echini*, by G. Valentin. Of the *Scutellæ* there are described thirteen genera and seventy-four species, illustrated by twenty-five quarto plates, inclu-

ding several hundred figures. These descriptions are preceded by a notice of the external and internal organization, mode of growth, relations to other Clypeastroides, and their geological position and distribution. On comparing the fossil and existing species, well marked differences have been found to exist, and of the genera Mellita, Rotula and Encope, all the species belong to the actual epoch. It is also interesting to notice the fact, that among the Scutellæ, as well as some of the other Echinodermata, the species beome larger and larger as we approach the present period, precisely the reverse of what is true with regard to some of the Vertebrata, Mollusca, &c.

Previous to the labors of Valentin, Tiedemann, Meckel and Delle Chiaje had already investigated the general anatomy of the Echini, but the microscopical examinations of the former into the minute structure of the different organs, are almost entirely new. In many of the soft parts, such as the ambulacral tubes, buccal membrane, external branchiæ, &c. Valentin has discovered small calcareous bodies, assuming various shapes, resembling somewhat the spicula described by many recent microscopists, as existing in the Sponges, Alcyonias, Actinias, &c.* "Why," asks M. Valentin, "may not these minute parts be preserved in a fossil state, as well as the shell, the lantern, the teeth, and other organs? I am convinced that the microscopic palæontology of the Echinodermata will become a vast field for research." The general organization of the shell, its microscopic structure, its mechanism, its appendages, and mode of increase, are all treated of in full detail, as are also the digestive, respiratory, circulating, nervous, and generative systems. Scarcely any thing is as yet known with regard to the mode of copulation, although the duality of the sexes has long since been determined. Nearly every thing relating to the embryology of the Echini, yet remains a desideratum.

The series of monographs of which those just noticed form a part, constitute one of the most important additions which have been made to modern zoology, no less in consequence of the completeness of the plan upon which they have been conceived, than the fidelity with which they have been executed. J. W.

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* Prof. Bailey has suggested the possibility of determining the existence of the Actiniæ in the tertiary formations, by means of the minute spicula, which are the only solid parts. Quite recently I have detected spicula in the dorsal appendages of the Eolis, which resemble somewhat those met with in the Actiniæ; they are found in a small sack, recently described and figured by M. Quatrefages, though no mention is made of its contents, situated at the extremity of the appendage, opening and discharging its contents externally.

3. *Description of New Fresh-water and Land Shells;* by ISAAC LEA.*—In the present paper Mr. Lea describes fifty four species of Unio, two of Margaritana, nine of Anadonta, one of Caracolla, one of Cyclostoma, and sixty of Melania, in all one hundred and twenty seven new species of land and fresh-water shells, described and figured by this indefatigable naturalist since the appearance of his last extended memoir on this subject, on the appearance of which it was supposed that this prolific subject was exhausted; as least so far, as that few new species of Naiades were to be looked for hereafter, and that future researches must bear mainly to the investigation of the anatomy and habits of the species already described. Some valuable light is thrown on this department of the subject in the present memoir, by the observations of Mr. Thomas G. Lea of Cincinnati, brother to our author, carried on during the years 1838, '9, and '40, on some of the species in the Ohio River, particularly in reference to the times of their parturition, which he finds to differ very much in the different species. Mr. Lea has tabulated his observations made on at least twenty five different species during each of the four years, but it will require careful and long extended observations to arrive at valuable results.

4. *Graham's Chemistry.*†—Prof. Graham's work is one of the best, if not *the* best, of all English text-books, on the difficult science of chemistry, and is of such recent date as to embrace the latest discoveries. The appearance of a correct and amended American edition under the care of Dr. Bridges, will prove an acceptable thing to both teachers and students of chemistry in this country.

MISCELLANIES.

FOREIGN AND DOMESTIC.

1. *Fossil Fruits described by Dr. Gideon Algernon Mantell.*—Dr. Mantell has recently read to the Geological Society of London, a memoir on three undescribed fossil fruits, from the chalk formation in the southeast of England.

(1.) *Zamia Sussexiensis*—a cone belonging to a plant allied to the Zamia, and found associated with coniferous wood at Selmestown in Sus-

* Transactions of the American Philosophical Society held at Philadelphia for promoting useful knowledge, Vol. VIII, new series, Part II, 1842. pp. 163–250, 4to.

† Elements of Chemistry, including the applications of the science to the arts, with numerous illustrations; by THOS. GRAHAM, F. R. S. Lond. & Ed., Professor of Chemistry, University College, London, &c. &c. With notes and additions, by ROBERT BRIDGES, M. D. Philadelphia, Lea & Blanchard, 1843. pp. 749, 8vo.

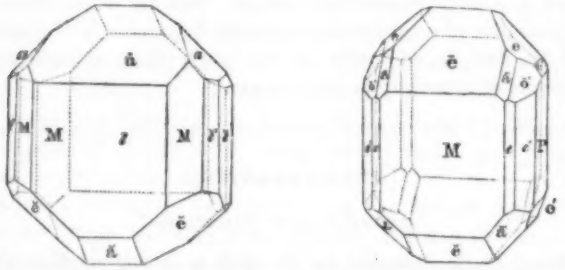
sex, in the bed described in Dr. Mantell's Fossils of the South Downs; this fruit is 5½ inches long.

(2.) *Abies Benstedi*—a fir cone found with coniferous wood in the Iguanodon quarry near Maidstone, and described in Dr. Mantell's memoir on the Molluskite, (see page 243 of the present volume.) This fossil contains numerous seeds in a fine state of preservation.

(3.) *Carpolithes Smithiæ*—a most remarkable fruit; it is the same that is described in Dr. Mantell's Fossils of the South Downs, from the white chalk near Lewes, as resembling a compressed nut of Areca. It was evidently a spurious compound berry, like the mulberry, the seeds imbedded in a pulpy substance. It was found by Mr. Smith, of Tunbridge Wells, in the white chalk of Kent.—(Letter from Dr. Mantell to the senior editor, dated Clapham Common, near London, March 30, 1843.)

2. *Eremite.*—A comparison of the angles of Eremite and Monazite, appears to indicate that these species are identical. They agree also in hardness, color, and lustre; the discrepancy in specific gravity may arise from imperfect determination, as error is scarcely avoidable in crystals so minute. The following are a few of the angles of Eremite. (See Am. Jour., Vol. xxxiii, p. 71.)

Monazite. Eremite.



M : *e*=136° 35′, M : ē=140° 40′, M : ĕ=126° 8′, P : e=131° 52″. For the corresponding inclinations, Monazite gives (Am. Jour. xxxiii, p. 203) 136° 30′ (*e* : M), 140° 10′ (ē : ā), 126° 25′ (ē : ă), 131° 22′ (ĕ : *a*). In a late article on the foreign Monazite by Delvoiseaux, in the Annales des Mines, t. ii, 1842, p. 362, these angles are given as follows: 136° 30′, 141° 5′, 126°, 131°. By calculation, M : T in Eremite gave the writer, 103° 46′, and in Monazite, 103° 42′. A specimen of Eremite in the hands of Mr. Thomas Dutton, shows a cleavage similar to that of Monazite. When first described, only three or four very minute crystals had been seen, and in these no cleavage was detected.

J. D. Dana.

3. *Meeting of the British Association at Dublin.*—This meeting of the Association was held during the month of August. It was a small meeting,—we do not learn that any thing very important came before it, and we are informed that party politics interfered somewhat with its success. One fact of great interest was announced. An upright trunk of a large Sigillaria has been discovered in the coal-field near Liverpool, with roots eight or nine feet long, spreading in every direction and with the radicles radiating from the main roots, and these roots and radicles are the *Stigmaria ficoides* and its leaves. This discovery must modify some existing theories on coal.*

4. *Animal of the Belemnite.*—Lord Northampton has recently obtained from the oolite of Chippenham, (Eng.) a specimen of a *Belemnite* with the impression of the soft parts of the animal on the surrounding clay! Even the little hooks with which the creature was furnished remain! Dr. Buckland's figure from D'Orbigny must therefore be modified; Prof. Owen, in the admirable volume of lectures on the Invertebrata, (the Hunterian lectures for this year, just published,) has given a restored outline of the animal of the Belemnite, and which must be correct.—(Extract from a letter to Prof. Silliman from Dr. Mantell, dated Aug. 28th.)

5. *Meteoric Epoch of August.*—In consequence of cloudy weather at this place for several days about the 10th of August, 1843, it was impracticable here to determine whether the meteoric sprinkle of August recurred the present year.

6. *Death of Mr. Bakewell.*—Robert Bakewell, Esq. died at his residence at Hampstead, near London, on the 15th of August, at the age of 75. He had long been an invalid, and his death was the result of gradual decline, rather than acute disease. Mr. Bakewell was one of the oldest of the present school of English geologists, and was the author of the first good treatise on geology in the English language, which went through five editions in England and three in this country, before the author's death; and it still holds a place among the best elementary works on the subject. Mr. Bakewell also published in 1823 two interesting and valuable volumes of travels among the Alps in Switzerland.

Mr. Bakewell's mind was distinguished for vigor, acuteness, and independence; his Geology was indeed much in advance of the science at the time he wrote, but his sagacious views have been fully confirmed. In the course of an epistolary correspondence of many years, we

* We hope to begin our usual abstract in our next number.

have found his letters rich in thought, and vivid and attractive in style, while a warm and true philanthropy imparted a living moral interest to his epistles.

A great degree of modest retirement characterized Mr. Bakewell's intercourse with society, and he carried it to such an extent as rarely to visit the sessions of any of the scientific bodies in London. This may serve to explain the fact that his treatise on geology was at first received with more favor in this country than in England.

7. *Death of Prof. Hall.*—Professor F. Hall, whose name has often appeared in our pages as a contributor of valuable matter, died during a journey at the west, in the month of August last. We have no information of the exact time of his death, nor his age, which however was not far from sixty. Prof. Hall was a zealous cultivator of mineralogy; he collected a large and valuable cabinet, which a few years since he generously gave to Dartmouth College, at Hanover, N. H., and at the same time he placed the chair of mineralogy in that institution on a permanent foundation, by the contribution of five thousand dollars in money.

8. *Death of Mr. J. N. Nicollet.*—It is also our painful task to record the decease of Mr. Nicollet, who died at Washington, D. C., on Monday morning, the 11th of September, a little after six o'clock, aged it is supposed about forty eight. Mr. Nicollet's labors in the departments of physical astronomy and geography are well known. He was the favorite pupil and friend of La Place; and the frequent occurrence of his name in the Mécanique Celeste, shows in what estimation he was held by his teacher.

Mr. Nicollet came to this country about ten years since, and has been engaged principally in carrying out a survey—geographical, topographical, astronomical, and geological—of the vast region embraced by the sources of the Mississippi and Missouri Rivers.

His map of this important labor was completed before his death, and was shown by him at the Association of American Geologists in April last, at Albany, and referred to in explanation of an interesting paper on the geology of the region in question, an abstract of which is contained in their proceedings, in the present volume of this Journal.

Mr. Nicollet also devoted much effort to the collection and preservation of the various Indian dialects, and in fact every thing which could illustrate the history of this interesting race. It is said his collections of MS. notes on this subject are quite voluminons.

All who had the pleasure of knowing him, and enjoying his fine social and moral qualities, will hear of his premature loss with deep regret.

ACKNOWLEDGMENTS TO CORRESPONDENTS, FRIENDS AND STRANGERS.

Remarks.—This method of acknowledgment has been adopted, because it is not always practicable to write letters, where they might be reasonably expected; and still more difficult is it to prepare and insert in this Journal, notices of all the books, pamphlets, &c., which are kindly presented, even in cases, where such notices, critical or commendatory, would be appropriate; for it is often equally impossible to command the time requisite to frame them, or even to read the works; still, judicious remarks, from other hands, would usually find both acceptance and insertion.

In public, it is rarely proper to advert to personal concerns; to excuse, for instance, any apparent neglect of courtesy, by pleading the unintermitting pressure of labor, and the numerous calls of our fellow-men for information, advice, or assistance, in lines of duty, with which they presume us to be acquainted.

The apology, implied in this remark, is drawn from us, that we may not seem inattentive to the civilities of many respectable persons, authors, editors, publishers, and others, both at home and abroad. It is still our endeavor to reply to all letters which appear to require an answer; although, as a substitute, many acknowledgments are made in these pages, which may sometimes be, in part, retrospective.—*Eds.*

SCIENCE.—FOREIGN.

Recherches sur les Poissons Fossiles. Texte et Planches. Liv. 14. 1841.

Histoire Naturelle des Poissons d'Eau Douce de l'Europe Centrale. Tome I, texte, contenant l'Embryologie des Salmones. Liv. 2, planches. 1841.

Etudes Critiques sur les Mollusques Fossiles. Liv. 2, avec planches, contenant les Myes du Jura et de la Craie Suisses. 1842.

Monographies d'Echinodermes, Vivans et Fossiles. Liv. 2, contenant les Scutelles; liv. 3, les Galerites et les Dysaster, par E. Desor; liv. 4, l'Anatomié du genre Echinus, par G. Valentin. Planches.

Nomenclator Zoologicus, continens Nomina Systematica Generum Animalium, tam viventium quam fossilium. Fasiculus 1, con-

tinens Mammalia, Echinodermata et Acalephas. Fasciculus 2, continens Aves.—With several prospectuses. All the above from the Author, M. Louis Agassiz, Neuchatel, Switzerland.

Recit d'une course faite aux Glaciers en hiver, par MM. Agassiz et Desor. From the Authors.

Aperçu general de la Structure Geologique des Alpes, par M. Studer; precede de quelques observations generales, par M. Desor. From the Author.

Letter to Prof. Forbes concerning his alleged discovery of the "lamellar structure of glaciers." From the Author, M. Agassiz.

Evolution of Light from the Living Human Species; by Sir Henry Marsh, Bart. M. D., M. R. I. A. Dublin, 1842.

Lectures on Agricultural Chemistry and Geology. Part II. By James F. W. Johnston, M. A., F. R. S.

The Effects of a Lightning Flash on the steeple of Buxton Church, and observations on Lightning Conductors generally; by Charles V. Walker, Sec. Lond. Elec. Soc. London, May 17th, 1842. From the Author.

On the Action of Lightning Conductors. London, July 19th, 1842.

Memoir on the difference between Leyden discharges and Lightning Flashes; by Charles V. Walker. From the proceedings of the London Electrical Society. London, Dec. 20th, 1842. From the Author.

Annuaire Magnetique et Meteorologique du corps des Ingenieurs des Mines de Russie, ou recueil des observations Magnetique et Meteorologique faites dans l'entendre de l'empire de Russie, et publiees par l'ordre de S. M. l'Empereur Nicolas I. Année, 1840–1842. Received March, 1843, from M. le Comte Cancrine, chef du corps des Ingenieurs.

Introduction to the Atomic Theory; by Chas. Daubeny, M. D., Prof. Chem. Oxford, 1841.

Elements of Agricultural Chemistry and Geology; by Prof. Johnson. Second edition. London. pp. 250, 12mo. From the Author.

Revue des Fossiles du Government du Moscow, par C. Fischer de Waldheim. No. 1, Belemnites. From the Author, forwarded by Charles Cramer, Esq.

The British Quarterly Journal of Dental Surgery, edited by J. Robinson, Esq. London, March, 1843. From the Editor.

Proceedings of the London Electrical Society. Session, 1842–3. Edited by the Secretary. April. From the Society.

Received April, 1843, from Signor Giovanni Michelotti, Turin, the following works, viz.

Brevi Cenni sullo condizione attuale della Sardigna, per l'Auv. Giovanni Michelotti. Torino, 1842. Ten copies.

Cenni Statistici sopra la Ricovero di Mendicita di Torino Nell Anno, 1841. pp. 36. Two copies.

Atti della seconda riunione degli scienziata Italiani tenuta en Torino. Nel Septembre, del 1840. pp. 400, quarto, pamphlet form.

Annali della scienze del regno Lombardo Veneto, opera periodica di Alenni collaboratori. 1841, Gennajo e Febbrajo, Marzo e Aprile, Maggio e Gingno, Luglio e Agosto.

De solaris in supracretaceis Italicæ stratio repertio. Auctore Jean Michelloti. Two copies.

Saggio storico del rizopodi caracteristici del terreni sopracreta cei, per Giovanni Michelotti. Modena, 1841. Quarto pamphlet. Five copies.

Saggio orittografico sulla classe del Gasteropodi fossili del terreni terziarii del Piemonte di Luigi Bellardi e Giovanni Michelloti. Two copies.

Catologo medaglio. Musio Numismatico Lavy appartnente alla R. Academia, della scienze di Torino. In two vols., 4to. pp. 500. Two copies.

Caroli Allionii Flora Pedimontana. Three volumes.

On the Distribution and Classification of the order of the Palæozoic Deposit of the north of Germany and Belgium, and on their comparison with formations of the same age in the British Isles; by Rev. A. Sedgwick, F. R. S., and Roderick I. Murchison, Esq., F. R. S. pp. 400, quarto, pamphlet form. With a set of plates. London, 1842. From the Authors.

Address of Mr. Murchison before the Geological Society of London, Feb. 18th, 1842. From the Author.

Maidstone; its Geology, History, Antiquities and Traditions, discussed in a memoir read in that town by Douglas Allport, Esq. From Dr. Mantell.

Address before the Royal Society on its anniversary, Nov. 30, 1842, by the most noble Marquis of Northampton, president of the Society. From Dr. Mantell.

Dent on the errors of Chronometers, and explanation of a new construction of the Compensation Balance. London, 1842. Five copies. From the Author.

Dent on the construction and management of Chronometers, Watches and Clocks. Three copies. From the Author.

The Royal Society, Fellows and Council. November, 1842. Quarto pamphlet, pp. 30. From Dr. Mantell.

Extract of a letter from Prof. Hansteen, Christiana, to Prof. M. Forbes, Edinburgh. Small pamphlet. pp. 8.

The Geologist; edited by Charles Moxon, Esq., from Jan. 1st, 1842, to Dec. inclusive. From the Editor.

On the specific inductive capacities of certain electrical substances; by W. Snow Harris, Esq., F. R. S. London, 1842. Quarto pamphlet, pp. 172. From the Author.

Arsberättelse om Technologiens framsteg till Kongl. Vetenskaps Academien afgifen den 31 Mars, 1840; af G. E. Pasch. Stockholm, 1841. pp. 24.

Kongl. Vetenskaps Academien Andlingar. För Ar 1840. Stockholm, 1841.

Tal af Academiens Præses Grefve M. Rosenblad. Stockholm, 1840.

Tal om Jordbrukets närvarande tillstand inom fadernestlandet, hundren för dess fooksfian och utsigterna for dess framtid. hallet 1. K. V. A. vid. Præsidii Nedlaggande den 6 April 1842, af August, Auckarsward. Stockholm, 1842, pp. 42.

Arsberättelser om nyare Zoologiska arbeten och Upptäckter, till Kongl. Vetenskaps-Academin afgifne för aren 1837–40. Af C. J. Sundewall. Stockholm, 1841. pp. 585. All from the Swedish Academy, through their perpetual Secretary, M. Jac. Berzelius.

SCIENCE.—DOMESTIC.

A Muck Manual for Farmers. Lowell, Mass. 1843. From the Author, Samuel L. Dana. 12mo. pp. 232.

Elements of Geology, with an outline of the Geology of North Carolina, for the use of students of the University; by Prof. Mitchell. 1842. Small octavo, pp. 141. From the Author.

Monograph of the Fresh Water Mollusca of the United States. No. 6. Two copies. From the Author, S. S. Haldeman, Genus Physa. Phil. 1843.

Farmer's Register, Vol. X, No. 12, containing an essay on calcareous manures; by Edmund Ruffin. Petersburg, 1842.

The Medical News Library, Vol. I, Nos. 1 and 2. Philadelphia. A continued series.

Transactions of the Society of Alumni of the College of Physicians and Surgeons of the University of the State of New York. No. 1, 1842.

Quarterly Summary of the Transactions of the College of Physicians of Philadelphia. 1843.

Transactions of the Am. Phil. Soc. Part II, Vol. 3. Observations on the species Unio. Two copies. From the Society, and I. Lea, Esq.

Binomial Theorem and Logarithms, for the use of the Midshipmen in the Naval School at Philadelphia. By and from Wm. Chauvenet. 1843. Small octavo, pp. 91.

Geological History of Manhattan or New York Island; by Issachar Cozzens, Jr. New York, 1843. pp. 114, small 8vo. From the Author.

Reports of the first, second, and third meetings of the Association of American Geologists and Naturalists at Philadelphia, in 1840 and 1841, and at Boston, 1842, embracing its proceedings and transactions. Boston, Oct. pp. 544.

Animal Chemistry, by Justus Liebig, M. D. Edited from the author's manuscripts, by Wm. Gregory, M. D.: with additions,

notes and corrections, by Dr. Gregory and others; by John W. Webster, M. D., Prof. Chem. in Harvard University, Cambridge.

A Catalogue of the Birds of Connecticut; by Rev. J. H. Linsley, A. M. From the Am. Jour. Science, Vol. XLIV, No. 2. From the Author.

Transactions of the American Philosophical Society for Promoting Useful Knowledge, held at Philadelphia. Vol. VIII, Part Third. 1843. From the Society.

Description of the American Limacidæ; by A. Binney, Esq. Boston. Three copies.

MISCELLANEOUS.—FOREIGN.

List of prices of mathematical and other apparatus, for sale by E. M. Clark, London.

Memoires sur le Canada depuis 1749 jusqu'a 1760, entrois parties avec cartes et le plans lithographies, 1838.

A number of bookseller's catalogues.

Twenty sixth report to the 20th November, 1842, of the London Provident Institution.

Sketch of the writings and philosophical character of Augustin P. De Candolle, Prof. of Nat. Hist. Geneva; by Prof. Daubeny, of Oxford. From the Author.

Austria; by W. R. Wilde, M. R. I. A. Dublin. From the Publishers.

Fourth annual report of the Morrison Education Society. Macao, 1842. From Rev. S. R. Brown.

Journal of a tour through the United States and in Canada, made during the years 1837–38; by Charles Daubeny, M. D., F. R. S., &c. Oxford. pp. 232, 12mo. From the Author.

MISCELLANEOUS.—DOMESTIC.

Tenth Annual Report of the Trustees of the State Lunatic Hospital at Worcester. Dec. 1842. Boston. From Dr. S. G. Howe.

Catalogue of Williston Seminary, fall term, 1842. East Hampton, Mass.

Twenty fourth Annual Report of the Directors of the New York Institution for the Deaf and Dumb. 1843. From H. P. Peet, Secretary.

Inaugural Address of the Hon. A. Gallatin, LL.D., on taking the chair as President of the New York Historical Society. New York, 1843. From the Society.

Reports to the Providence Athenæum, Feb. 1837 and Sept. 1838; by Stephen Trip, 1842. Do. submitted, Sept. 1842.

Seventeenth Annual Report of the Board of Managers of the Prison Discipline Society. Boston, 1842.

Transactions of the Hartford County Agricultural Society for 1842.

Northern Lakes as a residence for the invalids of the South; by Daniel Drake, M. D. Louisville, Ky. 1842. From the Author.

Annual Reports of the Interments in the city and county of New York, for 1842; by J. H. Griscom, M. D. From the Author.

Twenty second Annual Report of the Board of Directors of the Mercantile Library Association, Clinton Hall. New York, 1843.

Catalogue of Kemper College, 1842–3. St. Louis, Missouri.

Catalogue of Middlebury College, 1842-3. Middlebury, Vt.

New England and the West; by R. W. Haskins, Buffalo, N. Y. From the Author.

The Destinies of War and Labor. An essay delivered by A. E. Gwynne, before the Hamilton Chapter of the Alpha Delta Phi Society, 1842. Clinton.

Prof. Hitchcock's Anniversary Address before the Mount Holyoke Female Seminary. Amherst, Mass. 1843. From the Author.

Mr. Colman's Agricultural Address, 1842, at Rochester, N. Y. From the Author.

A Discourse on the rightfulness and expediency of Capital Punishments; by Rev. Wm. T. Dwight, Portland, Me. 1843. From the Author.

Gambier Catalogue for 1842–3. Gambier, Ohio. Two copies.

Circular and Catalogue of Willoughby University, 1842–3. Clinton, Ohio.

The Chicora, Nos. 9, 10, 11, 12. Charleston, S. C. From J. B. Legare, Esq.

President's Message and documents accompanying, to the 3d session of the 27th Congress. From Hon. S. J. Andrews.

Speech of Hon. Willis Hall in committee of the whole on the governor's message.

Report by Mr. Ferris on the electro-magnetic telegraph, Dec. 30, 1842. From Hon. F. Granger.

Report of Mr. Aycrigg on the Coast Survey, Feb. 9th, 1843. From Hon. J. Trumbull. From Hon. S. J. Andrews. From Hon. W. W. Boardman. From Hon. C. Morgan.

Report of the Commissioners of Patents, Feb. 1843.

Report by Hon. Mr. Pendleton, Ohio, on Military Posts, Council Bluffs, to the Pacific Ocean, Jan. 4th, 1843. From Hon. J. Trumbull.

Mr. Underwood's report relative to steamboat explosions. From Hon. W. W. Boardman.

Collection of the documents that appeared in relation to Dr. Sewall's drawings on the human stomach.

New England's Memorial, by Nathaniel Morton. Fifth edition, with notes, by John Davis. Boston, 1826. pp. 476, 8vo. From the Author.

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Remarks.—This method of acknowledgment has been adopted, because it is not always practicable to write letters, where they might be reasonably expected; and still more difficult is it to prepare and insert in this Journal, notices of all the books, pamphlets, &c., which are kindly presented, even in cases, where such notices, critical or commendatory, would be appropriate; for it is often equally impossible to command the time requisite to frame them, or even to read the works; still, judicious remarks, from other hands, would usually find both acceptance and insertion.

In public, it is rarely proper to advert to personal concerns; to excuse, for instance, any apparent neglect of courtesy, by pleading the unintermitting pressure of labor, and the numerous calls of our fellow-men for information, advice, or assistance, in lines of duty, with which they presume us to be acquainted.

The apology, implied in this remark, is drawn from us, that we may not seem inattentive to the civilities of many respectable persons, authors, editors, publishers, and others, both at home and abroad. It is still our endeavor to reply to all letters which appear to require an answer; although, as a substitute, many acknowledgments are made in these pages, which may sometimes be, in part, retrospective.—*Eds.*

SCIENCE.—FOREIGN.

Account of the Museum of Economic Geology and Mining Records Office established by government in the department of her Majesty's Commissioners of Woods and Forests, under the direction of Sir Henry De La Beche, F. R. S., F. G. S.; by T. Sopwith, F. G. S. London, 1843. From R. Phillips.

On the theory and construction of a Serismour, or instrument for measuring earthquake shocks, and other concussions; by James D. Forbes, Esq., F. R. S., &c. From the Transactions of the Royal Soc. of Edinburgh, Vol. XV, part 1, 1841. From the author.

On a remarkable structure observed by the author in the ice of glaciers; by J. D. Forbes, Esq., with a plate. Read before the Royal Society of Edinburgh, 1841. From the author.

Observations on the Aurora Borealis from Sept. 1834, to Sept. 1839; by Robert Snow, Esq. London, 1842. From the author.

Prof. Forbes' Account of his recent Observations on Glaciers; and A Fourth Letter on the Glacier Theory to Prof. Jameson; by Prof. Forbes. From the author.

Report of the Manchester Geological Society at their Fourth Annual Meeting, Oct. 27th, 1842, Jas. Heywood, Esq. in the Chair. From the Council of the Society.

On the Galvanic Properties of the Principal Elementary Bodies, with a description of a new Chemico-Mechanical Battery; by Alfred Smee, Esq. London, 1840. From the author.

On the Public Institutions for the advancement of Agricultural Science which exist in other countries; by Chas. Daubeny, M. D., F. R. S. London, 1842. From the author.

Tables for the extemporaneous applications of Corrections for Temperature; by S. Elliot Hoskins, M. D. Guernsey, 1842. Two copies from the author.

Lecture on the application of Science to Agriculture; by Charles Daubeny, M. D., F. R. S. London, 1842. From the author.

On the intimate rationale of the Voltaic Force; by Alfred Smee, F. R. S. London, 1842. From the author.

Memoirs and Proceedings of the Chemical Society, Part 2. From Mr. Teschemacher.

De mutationibus quas subit momentum virgæ magneticæ, partim ob temporis, partim ob temperaturæ mutationes; auctore Christophore Hansteen. Christianiæ, 1842.

Experimental Researches in Electricity, 18th series; by Michael Faraday, Esq. D. C. L. From the Philosophical Transactions, Part I, 1843. London. From the author.

On the transparency of the Atmosphere, and the law of extinction of the Solar Rays in passing through it; by Prof. Forbes. From the Philos. Trans., Part II, 1842. London. From the author.

Delphinus Leucopleurus, nova species, descripta ab H. Rasch, conservatore Musei Zoologici universitatis regiæ Fredericianæ. Christianiæ, 1843. Received August 27th, 1843.

Descriptio ornamentorum maximam partem aureorum et nummorum sæculi viii, vi. et ixni in prœdio Hœn, in parochiâ Eger in Diœcesi Norvegiæ Agershusiensi repertorum; auct. Chr. Andr. Holmhoc. Christianiæ, 1835. Received August 27th, 1843.

Semina Horti Botanici Christianiensis, 1842, collecta. From the Royal University. Christiana, Norway.

SCIENCE.—DOMESTIC.

Elements of Chemistry, including the applications of the Science in the Arts, with numerous illustrations; by Thos. Graham, F. R. S. With notes and additions, by Robert Bridges, M. D. Philad. 1843. Published by Lea & Blanchard. From the publishers. Large 8vo, pp. 749.

Reliquæ Baldwinianæ—selections from the Correspondence of the late Wm. Baldwin, M. D., U. S. N.; compiled by Dr. Darlington. Philad. 1843. 12mo. pp. 346. From H. C. Townsend, Esq.

Report on the exploration of the country lying between the Missouri River and the Rocky Mountains, on the line of the Kansas and Great Platte rivers; by Lieut. J. C. Fremont, of the corps of Topographical Engineers. Washington, 1843. pp. 243. From Col. Abert, and do. from Hon. Mr. Huntington, Norwich.

Lithotripsy, or breaking of stone in the bladder; by Alvan Goldsmith, M. D. New York, 1843. From the author.

Foreign Agriculture, No. I. The Economy of Farming, from the German of Prof. Benger, with copious notes from other authors; by E. Goodrich Smith. New York, 1843. From the author.

An historical sketch of the state of American Medicine before the Revolution; by John B. Beck, M. D. Albany, 1842. From the author.

Suggestions of new theories to the scientific; by A. Girard. Mobile, 1843.—With a newspaper containing an article entitled, Suggestions of a new Planetary System. From the author.

Observations of Encke's Comet at the High School Observatory, Philadelphia, March–April, 1842, with the Fraunhofer Equatorial; by S. C. Walker and E. O. Kendall. Read May, 1842. From the authors.

Contributions to the Geology of the Tertiary Formations of Virginia, second series; by Prof. W. B. Rogers and Prof. H. D. Rogers. Read March, 1839.

MISCELLANEOUS.—DOMESTIC.

Address to the Norfolk County Temperance Society at their meeting at Quincy, 29th Sept. 1842; by J. Q. Adams. From J. Harrington.

An Election Sermon, by the Rev. Samuel C. Jackson, Jan. 7th, 1843, before his Excellency the Governor of Massachusetts, John Davis, the Lieutenant Governor, and Common Council.

The Hierophant, or Monthly Expositor of Sacred Symbols and Prophecy; conducted by George Bush. No. III, Aug. 1842.

Report of the joint special committee of the Senate and House of Representatives of Massachusetts, to whom was referred the petition of George Latimer and more than 65,000 citizens of Mass. From Sam'l Greele. 1843.

Mid Lothian coal mining company's circular for 1843. Richmond.

Twenty-fifth annual report of the Asylum for the Insane, 1842. Philadelphia, 1842. From Dr. Pliny Earle.

Memoir of John Treadwell, LL. D., late Governor of Connecticut; by Prof. Olmsted, of Yale College. Pamphlet form, pp. 31. 1843. From the author.

The Dial. Nos. 12 and 13. Boston, April, 1843. From the Editors.

A Discourse at the ordination of the Rev. F. Butler, pastor of the Congregational Church in the East Parish of Windsor, Vt.; by Rev. J. Richards, pastor of the Church at Dartmouth College. 1843. From the Author.

Transactions of the Natural History Society of Hartford, No. I. Address on the birth day of Linnæus, May 24th, 1836; by Dr. S. F. Jarvis. Two copies.

Fifty-sixth annual report of the Regents of the University of New York, made to the Legislature, March 1, 1843. Albany.

Reply of Col. Abert and Mr. Markoe to the Hon. Mr. Tappan of the U. S. Senate. Washington, 1843.

Report of the select committee relative to the renewal of the State Railways with Pennsylvania cast iron rails; Mr. Trego, chairman. *April 4th, 1843. Harrisburg.*

"Facts for the People." *Cincinnati, O., March, 1842.*

Catalogue of fruit and forest trees for sale by Parsons & Co., Flushing, L. I. 1843.

Perkins Institution for the Blind. Annual report of the Trustees, 1843. Boston.

Report of the American Temperance Union, 1843. New York.

Report of the Am. Protestant Reformation Soc. N. Y. 1843.

First annual report of the Western Baptist Theological Institute of Covington, Ky. 1843. From E. Robins.

A new Grammar of the English Language. Boston, 1834. From the author.

Annual address before the Board of Trade of the city of Pittsburg, on Jan. 24th, 1842; by A. W. Loomis, Esq. Pittsburg, 1842.

Catalogue of the valuable Library of Mr. Town, for sale. Wiley & Putnam's Catalogues.

Army and Navy Chronicle. Washington, June 1st, 1843.

Collection of the documents which appeared in the public papers in relation to Dr. Sewall's drawings of the human stomach.

Dr. Pusey's Sermon. Published in New York.

Address before the Philological Institute, Dec. 8th, 1842, by T. J. Bigham, Esq. Pittsburg, 1842.

Order of Exercises at exhibition of Phillips Academy, Andover, Mass., August 8th, 1843.

Hunt's Magazine and Commercial Review, No. L, Aug. 1843.

An address delivered before the New Haven Horticultural Society, May 25th, 1843; by Alfred S. Monson, M. D., Pres. of the Soc'y.

A discourse on the duties and qualifications of an historian, delivered at the fourth anniversary of the Georgia Historical Society, Feb. 1843; by the Hon. M. King. Savannah.

Quarterly Journal of the American Education Society, Aug. 1843.

Catalogue of Western Reserve College for 1841–2. Hudson. From Prof. St. John.

Report on School House Architecture, made to the Board of Commissioners of Common Schools; by Henry Barnard, Esq. Hartford, 1842. From the author.

American Book Circular, with notes and statistics. 1843. From Wiley & Putnam, publishers, New York.

The progress and results of Emancipation in the English West Indies; by John Jay, of Bedford, N. Y. 1842.

The Snag Nullifier—description of an invention for preventing accidents by striking against snags. 1843. From the author, L. G. Mickles.

Annual circular of the Medical College of Louisiana, tenth session, 1843–4. New Orleans.

Annual announcement of the Jefferson Medical College of Phil. 1843–4.

Report of the joint special committee on the subject of the effects of lead pipes on well water in the city of Lowell; by Dr. S. L. Dana. 1842. *From J. W. Grant, Esq.*

Address delivered by Hon. Daniel Webster at the completion of the Bunker Hill Monument, June 17th, 1843.

Catalogus Collegii Hamiltonensis, 1843.

NEWSPAPERS.—FOREIGN.

The Nonconformist, London, July 12, 1843, from J. C. Dunlap, Esq.—A number of copies of the Scotsman and the Witness, Edinburgh, from J. Dunlap, Esq.—Jersey and Guernsey Advocate, April, 1843.

NEWSPAPERS.—DOMESTIC.

Albany Daily Tribune, March 17th, 1842, from Mr. Delavan.—New York Card.—Albany Patriot, from Mr. Delavan, containing some temperance discussions; a series of them, also of the Evening Journal.—Christian Freeman, Hartford, March 24th, 1843.—The Dayspring, Boston, June and July, 1842.—The Planters' Banner, Franklin, La.—The Clarion, Washington, D. C.—New York Daily Tribune.—Washingtonian Weekly News, New York, 1843.—Boston Semi-Weekly Courier, Feb. 1843.—The Literary Age, Feb. 1842, Philad.—Cincinnati Gazette.—American Messenger, Jan. 1843.—The Southern Chronicle, Jan. 11th, 1843.—The Midnight Cry.—The Protestant Vindicator, New York, Dec. 1842.—The Washingtonian Reformer, April, 1843.—Christian Intelligencer, New York.—The New World Monthly Messenger, Feb. 1842.—The Millennial World.—New York Spectator, March, 1843.—Republican, Savannah, Geo.—New York American, containing an article by Dr. Hare on lead pipes, June 9th, 1843.—Vicksburg Sentinel, Feb. 1843, with meteorological tables for that place.—Brooklyn News, June 3d, 1843, with a notice of a curious impression on stone

found in that city. Ohio Observer, Feb. 2d, 1843, containing a letter from Mr. E. Andrews to Prof. Barrows, Hudson.—The Constitution, Middletown, Dec. 1842, from Dr. Barratt, containing an account of the hard winter of 1779–80; from the same source, a notice of the various floods in Connecticut river.—Newark Daily Advertiser, May 2, 1842, from Prof. Henry, with a notice of the comet.—Indiana Statesman, Jan. 1843.—Georgia Messenger, Macon, June, 1843, from S. T. Bailey, with a notice of the phenomenon witnessed in Georgia and Florida on the same day with the great earthquake in St. Domingo.—Middletown Constitution, from Dr. Barratt, with a scheme for restoring salmon to Connecticut river.—Albany Argus, with an article on the progress of pledge taking.—Bloomington (Iowa) Herald, with the meteorological journal for 1842.—Fishkill Standard, Feb. 28th, 1843, with an article on gold fish.—Western State Journal, Syracuse, May 3d, 1842.—Norristown Free Press, with an article on comets, April, 1843.—Vermont Temperance Advocate, Feb. 1843.—The Comet, a Tract for the Times.—Owego Gazette, July 7th, 1843, with a notice of the "Owego Female Academy."—United States Gazette, Philad., Aug. 5th, 1843, with an article headed, "Cambridge Astronomers."—New England Puritan, Boston, July 28th, 1843.—The Inquirer, Philad., Aug., 1843, with a notice of the American Journal.—Commercial Advertiser, Buffalo, from R. W. Haskins, with some scientific memoranda translated from the French.—Journal and Advertiser, Auburn, 1843, noticing death of Rev. Jas. Richards.—The Sabbath Vindicator, New York, Aug. 1843.—Pittsburgh Gazette, July 27th, with a notice of this Journal.—The Gazette, Boston, Aug. 26th, 1843.—The Christian World, Boston, July 22d, 1843, with a notice of Washington Allston.—Boston Semi-Weekly Gazette, from R. H. Dana, Jr., Aug. 21st, 1842, with an article headed "Mr. Sturgis' Letter."—New England Farmer, from Dr. C. T. Jackson, with some remarks on "Common Salts as Fertilizers," Boston, May, 1843.—New York Sun, Aug. 29th, 1843.—Albany Evening Gazette, July 7th, 1843.—Albany Atlas, Aug. 21st, 1842, from Mr. Delavan.

SPECIMENS.

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A suit of Pliocene Shells from the Tertiary of Italy, near Piedmont. From M. Avocat Jean Michelotti, à Turin.

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A suit of fossils from Mount Lebanon, Syria, embracing some interesting fishes, similar to those from the yellow limestone of Monte Bolca, Italy. From Rev. E. R. Beadle.

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